

# ANNUAL REVIEW OF PSYCHOLOGY

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## PREFACE

A measure of the increasing worth of a journal has traditionally been the growth in the size of its circulation. While such a criterion has much merit, and the *Annual Review of Psychology* receives a good mark by this standard, its editors and Editorial Committee members are far more concerned with what its readers think and write about it. Of course, a number of journals give each volume brief reviews, and the editor of *Contemporary Psychology* approvingly notes the annual appearances. But, prior to this year, only Lyle Lanier has given the *Annual Review* the careful scrutiny a much longer review allows ["An evaluation of the *Annual Review of Psychology* (Volumes I-IV)," *Psychological Bulletin*, **51**, 180-89 (1954)]. The year 1960, however, has seen two other rather extensive critiques, both by Hyman Meltzer and both directed toward the chapters in the area of industrial psychology. These appear as "Review of reviews in industrial psychology, 1950-1959," *Personnel Psychology*, **13**, 31-58 (1960), and as "Scope of industrial psychology references in the *Annual Review*, 1950-1959," *Journal of Psychology*, **49**, 43-56 (1960). The Editorial Committee welcomes these articles and hopes that they will stimulate other psychologists to consider as carefully the other areas the *Annual Review* attempts to cover.

The so-called master plan of chapter organization, described in the Preface of Volume 11, is now fully in operation, although subject to annual scrutiny and revision. One revision, suggested by several readers and agreed upon by the Editorial Committee at its 1960 meeting in Chicago, includes the offering of an occasional chapter on counseling. While the Committee felt that, as a general rule, this area would be reasonably covered by the several chapters in the abnormal and clinical area (see the psychotherapy chapter in the present volume), an occasional chapter devoted more specifically to counseling problems would seem advisable; hence, such a chapter will appear in Volume 14. It proved impossible to present a chapter on the status of psychology in the U.S.S.R. in this year's *Annual Review*. But Josef Brozek will prepare this chapter for Volume 13.

This year's subject index has been somewhat enlarged, and the policy of printing five-year cumulative chapter and author indexes has been continued. A minor change in format comes with the introduction, in all publications of Annual Reviews, Inc., of double-column bibliographies.

The Editorial Committee has lost the valued services of Dale B. Harris whose term expired January 1, 1961, but has gained in the addition to its membership of Kenneth E. Clark who took office in January of 1960. During the early part of the year Miss Marian Hays served as editorial assistant and, later, Miss Jeanine Ardourel succeeded her. Again this year, the Misses Dorothea and Sheila Ross compiled the subject index. The editors and mem-

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bers of the Editorial Committee deeply appreciate the services of these four persons. Attention is called to the fact that it is the Editorial Committee of 1958 whose members (listed on page ii) were responsible for the selection of the chapter topics and authors of the current volume.

D. B. H.	K. E. C.
J. McV. H.	O. W. McN.
L. G. H.	Q. McN.
H. S.	P. R. F.

## ERRATA

### Volume 11

page 99, line 6; page 100, line 22; page 121, reference 59: *for Zegers read Zeghers.*

page 206, line 8: *should read . . . F-tests if there is a suitable selection of degrees of freedom.*

page 209, lines 32-35: The characterization of Sawrey's article was misleading; it was a balanced discussion.

TOPICS AND AUTHORS  
ANNUAL REVIEW OF PSYCHOLOGY  
VOLUME 13 (1962)

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NEUROPHYSIOLOGY OF LEARNING, *G. J. Thomas*  
LEARNING THEORY, *W. K. Estes*  
ETHOLOGY, *M. Lindauer*  
SPATIAL VISION, *R. M. Boynton*  
AUDITION, *E. G. Wever*  
PERCEPTION, *H. W. Hake*  
MASS COMMUNICATION, *W. Schramm*  
PERSONNEL MANAGEMENT, *M. D. Dunnette*  
CONSUMER ANALYSIS, *R. L. French*  
STATISTICS, *A. Lubin*  
PSYCHOTHERAPEUTIC PROCESSES, *H. Strupp*  
PSYCHOLOGICAL DEFICIT, *R. M. Reitan*  
PERSONALITY DYNAMICS, *A. Jenness*  
SOMESTHETIC SENSES, *J. P. Nafe and D. R. Kenshalo*  
EDUCATIONAL PSYCHOLOGY, *F. W. Warburton*  
CURRENT DEVELOPMENTS IN SOVIET PSYCHOLOGY, *J. Brozek*

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## DEVELOPMENTAL PSYCHOLOGY<sup>1,2,3</sup>

BY HENRIETTA T. SMITH AND L. JOSEPH STONE

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It has become customary for each temporary occupant of these pages to offer views on the vigor and productivity of developmental research, possibly projectively supplying evidence of his own biases and unfulfilled yearnings. Risking such exposure, the writers, despite differences in theoretical outlook and training, join in expressing dismay mingled with optimism over the annual outpouring of papers. Our dismay arises from papers of an arid scientism [noted by Jersild (155)]; from "theoretical" papers dealing with theory at a level of medieval disputation, insusceptible to real generalization; from clinical reports riddled with *post hoc* reasoning and dubious assumptions. It is obvious that there is need and hunger for genuine theoretical advance and the opening of new research areas: it is dismay-worthy that the most productive and influential hypotheses have entered the field from outside, indeed, most often, from outside psychology itself. Optimism, on the other hand, is warranted by papers in which definitions and assumptions are clarified (by careful analysis, not pseudo-operationism), by increasingly sophisticated recognition of effective variables other than those being experimentally manipulated, by evidence of the wedding of fruitful theory to sound method, and by growing attention to the "intervening variables" recently pointed to by Baldwin in these pages.

Insufficient attention has been paid, as yet, however, to the principle of levels and the inescapable emergence of genuinely new phenomena at higher phyletic levels or at more complex levels of organization [Frank (155)]. Similarly, differing theoretical approaches, or, more properly, differing traditions (neobehaviorism, psychoanalysis, ethology, biopsychology, educational psychology, and the others) still rarely intersect effectively despite their mutual concern with children or childhood or development.

Despite the tremendous and obvious value of animal research on development, many studies intended to develop comparative principles and regularities fail to take account of species-relevance. To require a human being to spin a web or peck at corn would be hardly less appropriate than some of the tasks carried over directly from, say, rats or pigeons, which have been

<sup>1</sup> The survey of the literature pertaining to this review was concluded in March, 1960.

<sup>2</sup> The writers are grateful to their students and to their colleague, Margaret S. Faust, for assistance in preparing this survey.

<sup>3</sup> Abbreviations used in this chapter include: CMAS (Children's Manifest Anxiety Scale); MMPI (Minnesota Multiphasic Personality Inventory); PARI (Parental Attitude Research Instrument); TAT (Thematic Apperception Test); UPA (unqualified power assertion); WISC (Wechsler Intelligence Scale for Children).

set for children. Some of these seem more like studies of how to limit conditions so children cannot learn than attempts to determine the true dimensions of capability. The operation of Maier's Law (125) may be suspected! Many studies designated by title or by journal as dealing with children simply use children as a handy sampling of human Ss and have nothing to offer about childhood or about developmental characteristics. Most of these are omitted from consideration.

#### MOTIVATION

Rather belatedly, perhaps, in view of similar enunciations by Murphy and others 25 years ago, there seems to be a determination to free developmental psychology and psychology in general from the shackles of tissue needs and homeostatic motivation theory. In a paper of fundamental significance White (193) speaks of "competence" as the common factor in all the visual exploration, grasping, locomotion, linguistic, and other activities involving effective interaction with the environment. Such "motivation shows itself most unambiguously in the playful and investigating behavior of young animals and children." Harlow, with Zimmermann (77), restates the primacy of contact over nutritive motives in the development of the infant monkey's affectional attachments. From the same laboratory, Terrell (189) presents findings pointing to an external "manipulation incentive." To the writers it does not seem necessary to displace all such motivations onto the environment or eschew any concept whatever of inner drive (i.e., motive) simply to avoid the traps and limitations of drive-reduction theory. (Terrell's demonstration of the importance of manipulation as a successful incentive is marred by the fact that it involves the candy reward whose effectiveness is being denied, even though in imagined form. Manipulation, instead of the objects to be discriminated, might have provided a less tainted variable.) Mason, Harlow & Rueping (128) showed that manipulatory behavior in the rhesus took place during the first 10 to 20 days of life and that handling of a variety of conspicuous objects in the cage became progressively more frequent for at least 60 days. This behavior, too, received no external reinforcement and showed no relationship to feeding or other "primary drives."

Rheingold (7), concerning herself with human infants, likewise denies the primacy of oral needs and suggests (again bringing to the fore views long ago advanced and apparently long since forgotten) that infants actually seek arousal from their environments. In a similar—if perhaps lighter—vein Morison (136) comments on classical learning theory's doubt that animals ever . . .

. . . do anything except when motivated by a base desire for food, or sex, or a craven wish to avoid pain. There is no obvious place for play activities or for maneuvers which increase, rather than decrease, elemental drives—courtship for example.



Comparable views are advanced by Rethlingshafer (159) and by Butler (25). Martin (126), too, points to "the growing protest against the invoking of drive reduction as the primary, if not the exclusive, dynamism of behavior . . . [as] . . . more and more in disagreement with our observations of children." For him (as, apparently, for White and for others) the stress on striving to master the world leads to a cognitive theory of behavior and development—indeed, one might say, to a cognitive theory of conation.

The *Nebraska Symposium on Motivation 1959* (95) contains an important array of materials summarizing significant viewpoints; the current volume should not be overlooked by those concerned with development.

#### RESEARCH METHODS AND INSTRUMENTS

Methodologically speaking, it may be noted that instruments often generate research. The Parental Attitude Research Instrument (PARI) and the Children's Manifest Anxiety Scale (CMAS) appear to be having such effects. Freedheim & Reichenberg-Hackett (59) and Zuckerman (198) worked with checking the validity of the PARI and proposed a new form to increase its validity. The series of publications on the Test Anxiety and General Anxiety Scales developed at Yale is continued this year in Davidson's (44) report using mothers' interviews both as validating criteria and as sources for understanding the developmental anxiety in children. Possibly the most interesting finding, in addition to another validation of the scales, is that these scales seem to be more applicable to boys than girls. In contrast, the study by Phillips, Hindsman & Jennings (151) shows that boys do not admit anxiety as easily as do girls, at least as measured by dissatisfaction with themselves. Another result from this study, supporting findings of Hafner & Kaplan (73), is the negative correlation between intelligence and anxiety. Ruebush (170) finds that highly anxious children of low and middle levels of intelligence perform better on tests requiring a cautious approach than do children who are high in both anxiety and intelligence. Thus, it is insufficient to study the effects of anxiety on performance without also considering intellectual levels and nature of task involved.

Further validation of established tests continues. Henry & Farley's (84) Study of the validity for adolescents of the Thematic Apperception Test (TAT), with "blind" interpretations, is a cautious, careful, and ingenious support for this test. Hartup (82) evaluates the Highburger Early Adjustment-to-School scale, finding that it has utility as a long-term predictor of children's group behavior. Maxwell (129) concludes from his factor analysis of the Wechsler Intelligence Scale for Children (WISC) that the two independent factors are verbal-intellectual and spatial-performance. Littell's (117) review of the research literature on the WISC suggests that the areas most needing research are the test's predictive validity, its theoretical framework, and systematic effects of such variables as practice effect and circumstances of the examination. Lehmann (109), studying urban-rural

differences on Form L of the revised (1937) Stanford-Binet, says that this form needs further revision. How well the newly published Form L-M will meet his criticism is still to be explored.

The Rorschach test has been the focus of several studies, which include suggested new uses. Two studies of Rorschach variables, one of card rejection as a function of intelligence [Tolor, Glass & Mermelstein (190)], and the other on relation among types of movement, *M*, *FM*, and *m* responses [Reisman (158)], fail to confirm hypotheses previously developed for adults. The failures to replicate may prove to undermine the original hypotheses, or may turn out to constitute further caution against uncritically applying to children principles of interpretation which hold for adults. Ledwith (108) has provided a welcome compilation of age, sex, and intelligence norms for the Rorschach (Klopfer scoring) for children from six through 11, based on almost 1000 protocols, most of them obtained serially but with additional groups to provide control data on the effects of repeated testing.

Phillips, Kaden & Waldman (153) review the use of the Rorschach as a tool for the genetic analysis of perception and offer a revised quantitative scoring system for this purpose. Smith & Phillips (181), using this measure at several stages of preadolescent and adolescent development, found that social maturity (Vineland) scores were related to perceptual-cognitive level but in a complex fashion, which itself underwent changes with age. Lorenz (118) proposes ways by which the language used in Rorschach responses reveals the perceptual modes of the individual.

Other new uses for established techniques include Clawson's (31) work showing that the Bender-Gestalt test can be used diagnostically for emotional disturbance. Blau & Schaffer (13) have devised a means by which spiral aftereffect can predict electroencephalogram records.

Several new tests have been constructed. A device to measure "mental health" in infants is offered from Toronto [Flint (57)] under the title of The Infant Security Scale; it is "a diagnostic checklist" covering the first two years of life, with work already planned toward its further refinement. Berwick (12) offers a vocabulary test based on the use of words with several meanings which is said to be valid and reliable and more efficient than other vocabulary measures. Clifford (33) reports that preschool-aged children are not capable of responding to a paired comparison procedure.

A wide variety of new tests have been devised in the area of personality: Rosenberg & Sutton-Smith's (164) scale of masculinity and femininity; Goldfarb's (66) normality ranking of psychiatric patients; Schaefer, Bell & Bayley's (173) rating scales for maternal behavior; Hoefflin & Kell's (87) incomplete sentence blank for parent-youth relations; Coopersmith's (38) method for measuring self-esteem; and Sutton-Smith & Rosenberg's (188) scale of impulse behavior. Cattell (26) and a group working with him (27, 28, 34) have developed a series of objective tests to measure personality structure over the age range from four to eight years.

A re-emergence of observational techniques can be seen in the revived interest in controlled observation. The symposium led by Prall (154) on work with emotionally disturbed children has much to contribute to all observational research. Considering such issues as where, when, and how to observe, the quantification of observational data, and direct and inferential observations, the participants point out not only the problems inherent in such an approach, but possible ways of coping with them. In addition, Clifford (32) reports that discipline in the home can be studied adequately by using parental observations and records as guided by daily information sheets and descriptive records.

#### GROWTH

Continued interest in physical growth patterns is evidenced. Bayer & Bayley (6) offer a practical guide for diagnosing and predicting growth, based on the Berkeley and later data. Kagan & Moss (98) find that whereas maternal education is more highly correlated than paternal education with children's IQ (supporting the earlier Berkeley results), the height of children correlates higher with paternal than with maternal height. Govatos (68) indicates that in elementary-school children some specific aspects of physical growth are accompanied by specific motor skills (e.g., dental and grip age are significantly related to jumping and reaching) and that gross physical growth is correlated with motor skills in general. According to Simon's evidence (178), growth maturity is correlated with academic success for children between  $4\frac{1}{2}$  and  $7\frac{1}{2}$  years of age.

Faust (53) has studied the relationship of physical maturity to prestige in adolescent girls. Extending Tryon's earlier study, she finds that precocious physical growth is a detriment in prestige status during the sixth grade, but an asset during the next three years. Although the level of physical development is not the sole factor determining status, this study and Simon's indicate some of the possible psychological correlates of physical development.

#### EFFECTS OF EARLY EXPERIENCE

While it is perhaps too much to hope that all writers of popular articles or even of texts will henceforth refrain from speculative discussions of Kamala and Amala, be it recorded that the year witnessed the publication of a long, sad report (143) of the failure of patient effort to verify any part of the famous story of feral upbringing or even to identify the place the children were said to have been found. A linguistic note suggests that the "wolves" may have been "tigers." About all that can be said is that it seems probable that there were two highly limited, nonverbal little girls in an "orphanage" under the care of a not overly trustworthy Mr. Singh. However, with the growing research interest in deprivation and various other manipulable variables of early experience, we are at last moving out of the period of dependence on such ill-supported accounts.

*Sensory deprivation and enrichment.*—A group of studies in animal development deals with the later effects on form recognition of early sensory deprivation or enrichment. Representative studies are those by Gauron & Becker (61), Woods (197), Gibson, Walk & Tighe (62), Meier & McGee (132), Dashiell (42), all done with rats. Collectively, the findings are ambiguous. One reason for the ambiguity may be contained in Dashiell's study of the maze behavior of rats blinded before their eyes opened, which suggests that vision may play a very minor role in the spatial orientation of the rat.

Riesen (162), in a remarkable report of data obtained over a period of years on young, light-deprived chimpanzees and kittens, found chemical and atrophic changes in the retina which became irreversible when the deprivation continued beyond infancy. The effect was more marked in the higher mammals. Such evidence, made possible by refined techniques, calls for considerable revision of our concepts of maturation and function and re-examination of earlier negative findings in this area. Griffiths' (72) work on the effect of isolation and stress on rats' escape thresholds, points up the relationship between deviant sensory environments (restricted or intense sensory input) and specific thresholds. Rats subjected to stress stimuli and rats isolated from stress stimuli have higher thresholds for shock than rats reared under normal conditions. Denenberg & Bell (45), varying the age at which different levels of shock were administered to infant mice, found that shock during infancy facilitated avoidance learning in adults if the magnitude of shock had been low, but interfered with it if high.

Guinea pigs raised with experience from birth of a usually-rejected bitter substance, painted on the mother's nipples or constantly supplied in the drinking water, failed to show a lasting modification of the aversion response (192). Meier & Stuart (133) found that "gentled" Siamese kittens differed from their controls in speed and depth of coloring, a finding which they interpret in terms of hastened maturation as a result of hormonal and neurophysiological changes produced by the "gentling."

Pfaffenberger & Scott (150) working with puppies from birth found that the critical period for learning and adjusting to new physical environments occurs at 12 weeks of age. If, after this age, the puppies are ignored except for necessary care and do not have a rich environment, they are unable to take responsibility, as evidenced by their failure to complete guide-dog training successfully. It is interesting that Pfaffenberger (but not Scott) believes their reactions are comparable to those of children reared in orphanages, in both areas a break in the socialization process being responsible for such results.

Seitz (175) removed kittens from their mothers to individual isolation cages at two weeks, six weeks (age of spontaneous lapping), and 12 weeks, respectively, and at nine months compared their responses in a number of test situations. The two-week group apparently showed more alertness,

"anxiety" in novel situations, and aggression. They were less successful in obtaining food under competition and were slower in learning. Some developed an asthma-like state. The (overprotected) 12-week group seemed lacking in fear response but developed severe disturbance in the shock situation. Replication with greater sophistication in data treatment is desirable. Dennis' (46) study of three groups of children in Iranian institutions continues to de-emphasize the importance of emotional relationships during infancy for physical development. Rather, he stresses the role of experience and practice as crucial in maturation and development.

The effects of specific variables in mothering procedures have been experimentally manipulated by Rheingold & Bayley (160). Eight of 16 institutional babies were given the concentrated attention of one person from the sixth to the eighth month of life while the others received multiple ministrations. The children, later placed in homes, were tested for social responsiveness at 19 months. Significant differences in favor of the single mother-surrogate group were found only in the area of vocalization. All the children showed satisfactory development with no evidence of deprivation symptoms.

Rheingold [cited in (155)] finds evidence (essentially in support of the view taken long ago by Clarence Day in *This Simian World*) that "in the human infant the characteristics of social responses . . . are species-specific and biologically inherent."

The sensitivity of young primates to social isolation or contact was evident in two studies. One (83) showed that children left alone by E for even 10 minutes displayed more aggression than other Ss in immediately ensuing doll play. In a new report from the Wisconsin laboratories (127) it was demonstrated that peers, even strange ones, had a notably calming effect on emotionally upset young rhesus monkeys—an effect, it is characteristically stressed, in no way dependent on earlier nutrient activities.

*Imprinting.*—During the past year Hess (85) has reviewed a number of studies of imprinting and has published (86, 95) further evidence on the nature of the critical period, following the procedures he has been developing to permit the isolation and quantification of a number of the factors involved in imprinting. The onset of fear responses is held to limit the period of imprintability. Differences among species and breeds of birds are also developed.

*Human infancy.*—Levy (113), studying infants' earliest memories of inoculation, reported that by six months of age the child may show some remembrance of past inoculations, but only if these occurred one or two months prior to testing. By 12 months of age, however, the child can remember inoculations which occurred three or four months earlier. Smillie's (180) work on 230 infants during the first two years of life showed that although 75 per cent of them did not maintain a constant ratio of height to weight, only 16 per cent of these deviations were of a serious nature. Other studies concerned with an evaluation of neonatal functioning as a means

for finding stable patterns of behavior include Wolff's (196) observations of sleep patterns; Rosenblith & Lipsitt's (166) work with the Graham scale; Lipsitt & Levy's (115) measurement of electrocutaneous thresholds as related to increasing age; and Lipton, Steinschneider & Richmond's (116) attempts to observe the effects of motor activity on physiological, especially autonomic, states. All of these are concerned with the possibility of finding precursors for later normal or abnormal functioning in areas such as perception and motivation. Fish (54) made longitudinal observations of infants in order to investigate possible constitutional factors related to schizophrenia, and found that for one child neurological and physiological maturation was disrupted as early as one month of age and differed significantly from normal, retarded, and precocious patterns of development as shown by other infants.

#### PARENT-CHILD RELATIONSHIPS

A continuing research trend is evident in systematic, empirical studies of the effects of various parental practices and attitudes on a variety of dependent variables in children's behavior, attitudes, or personality traits. (See also Disturbances of Development section, below.) Harris (78) found that within the normal range the best "adjusted" children were those whose mothers were most emotionally mature. Other studies confirmed well-foreshadowed hypotheses that parental dominance tended to produce strong ethnocentrism [Dickens & Hobart (48)], and that parental coerciveness led to strong needs for self-assertion and, in the presence of high autonomy, successful assertion in school [Hoffman, Rosen & Lippitt (88)]. Multiple correlation of mothers' and fathers' rigidity with that of their four- to five-year-olds was around .50 [Blum (17)]. But mothers who are inclined, on the basis of current research, to feel responsible for whatever their children become may take heart from one of a projected series of reports by Chess, Thomas & Birch (29), which offers impressive evidence of personality ("reaction-type") consistencies from two months to 24 months despite wide differences in methods of maternal care.

Lynn & Sawrey (122) studied the effects on Norwegian children of the absence of their sailor fathers through a structured play interview, comparing boys with girls and father-absent with father-present families. Boys (but not girls) with father absent showed greater immaturity, stronger striving for father identification, and poorer relations to peers. More girls than boys in the absence of the father showed increased dependency on the mother. In the light of apparent differences in the two groups of mothers and of the high specificity of the play items in the criterion measure, further research would appear desirable, manipulating father vs. mother absence, the measures of maturity and independence, cultural factors, etc.

Rosen & D'Andrade (163) made use of an ingenious laboratory-method-in-the-home to study simultaneously boys' achievement in prob-



lem solving and parents' behavior while watching their boys at work. Among other findings: parents of boys with high achievement needs are more competitive and more involved, set high expectations of excellence, and meet good performance with warmth, poor performance with disapproval. Kagan & Moss (97), reporting on children for whom TAT, Rorschach, and Binet protocols were available over a period of years, along with information on parental education and observations of early maternal behavior, found that early maternal concern with achievement produced increased achievement fantasy (and IQ) in girls. Achievement fantasy tended to be stable over time though increasing in quantity with age. Research by Siegel *et al.* (176) on a small number of five- to six-year-old children of working mothers revealed no significant differences between them and their carefully matched controls with nonworking mothers. Evidently employment is not in itself an important determiner of the quality of the mother-child relationship. A questionnaire study [Nye (141)] of over 2000 children found no differences between children of working and nonworking mothers except for a small but significant tendency for more delinquent behavior in the former.

#### EFFECTS OF SOCIAL AND CULTURAL DIFFERENCES

*Cultural factors.*—It is encouraging to find field studies of growing theoretical and methodological sophistication that are obtaining concrete, directly comparable cross-cultural information [e.g., Anderson *et al.* (1)], but that avoid many of the traps of genotypic-phenotypic confusion which marred much of the earlier work; specific behaviors are more likely to be seen in the total cultural context. Green (70), in an analysis of recent publications, compares the economically similar segments of Jamaica and Puerto Rico and finds the essential differences in child rearing to stem from English vs. Spanish cultural traditions of the two peoples. Particularly, these are seen as causing "profound differences in nurturance and independence training. . . ." A volume by Landy (106) reports an ethnographic study of rural Puerto Rico explicitly focussed on child-rearing practices; direct comparisons with Sears' New England studies are included.

Danziger (40, 41), offering quantification of interviews with Javanese mothers, reports that professional-class mothers stress independence training and subjection to imposed duties far more than working-class mothers. The latter are considered to be closer to traditional Javanese ways, with the child and mother "part of a collectivity," whereas the professional group, under the greater influence of Western values and education, is coming to see the child "as a separate individual with rights and duties of his own." Little information is given, however, on class differences in Java prior to Western influence.

Two presumably very similar cultures were examined [Brown (23)]

through administration to 12- to 13-year-olds in New Zealand of the "Day At Home" questionnaire, which attempted to determine the authority structure, sources of tension, etc., in the family—a questionnaire previously given in Australia. There was apparently more joint decision and greater flexibility in New Zealand.

A posited, sharp distinction between paternal and maternal roles in Filipino, as compared with American, culture is found by Rabin & Limuaco (156) to be reliably reflected in the drawings of over 100 10- and 11-year-olds of each group; Filipino children more sharply distinguish between the sexes in their drawings. One might doubt, however, that this necessarily means for the American children a blurring of sex identification.

*Social class factors.*—Some indication of the separate influences of the family's social class membership and of peers in high school is provided by Wilson (194), who showed that sharp differences in the college aspirations of children with different parental occupations were considerably modified if in high school they were exposed to children of contrasting background. The results are taken as pointing to the dangers of class segregation where school assignment follows sharp neighborhood boundaries. Stewart (186) finds evidence that children may act independently of class expectations which they clearly perceive.

Children's own perception of class differences was studied with the use of an intriguing method by Jahoda (92) in Great Britain. Children were required to make two pictures out of four pieces. Although both possible pairings looked correct, only one was a socially "correct" matching of families to home background. There was a regular increase in correct matchings from ages six to 10. Results were closely related to intelligence. Working-class boys were somewhat more alert than girls to class differences. This was reversed for middle-class children, who tended to see their own level as the norm and to treat the working class in terms of condescension. The author is not wholly satisfied with the drawings which may have exaggerated working-class poverty.

Kohn's (105) study of 400 working- and middle-class parents of Washington, D.C., fifth graders found similar amounts and types of punishment reported. However, working-class parents tended to punish bad consequences of disobedience, whereas middle-class parents punished on the basis of their perception of the child's intent. The implications for the study of children's ideas of justice should be obvious.

Bayley & Schaefer (8), using actual observations, reported only a "slight tendency for the mothers of higher socioeconomic status to be more warm, understanding, and accepting" either in the first three years or when their children were nine to 14 years old. Their sample, though broad in range, was predominantly middle class in origin. Their comment that mothers are not much influenced by the advice of "authorities" on child



care should be seen in the light of the fact that the second observation period was from 1939 to 1942, relatively early in the swing to a psychiatrist or Spockian view; nor is any comparison offered of the prevailing patterns of behavior of new mothers in each period. Although old dogs may not learn new tricks, it is possible that their daughters will.

In a related vein, Hoffman (89), using rather small samples, compared detailed parental reports of the preceding day's interactions with their children with observations of the children in nursery school. The parental data were examined particularly for "unqualified power assertion" (UPA) over the child; the observations were examined for hostility, power assertion, and resistance. Working-class fathers used more UPA than middle-class fathers or working-class mothers, and Hoffman speculates that the mothers may be more open to the influence of expert opinion. Frequent use of UPA with a resistant child produces in him higher hostility and power assertion.

Lesser's (112) empirical examination of earlier reports that aggression was socially rewarded in lower-class children revealed that such children sharply discriminated among various forms of aggression, with provoked physical aggression most accepted and verbal and indirect forms least tolerated (reversing adult order of acceptance).

#### PERSONALITY TRAITS

*Identification.*—This apparently is the year in which concern with the problem of identification has resulted in unusually clear theoretical and experimental exploration. With increasing interest in the concept of identification much research has been conducted, but, as Bronfenbrenner (20) points out, there has been little agreement or consensus relative to its definition. He differentiates three aspects of Freudian identification (behavior, motive, and process) which must be clearly recognized for meaningful research. Bronson (22) differentiates infantile and ego identification, and elaborates the antecedent conditions relevant to each. Rau (157) stresses the relationship of the parent-as-model (specifying attitude, behavior, and roles) to identification, as well as advocating that sex-typing be regarded as probably a quite different phenomenon for which identification may be necessary but not sufficient.

From these theoretical presentations, several areas of concern can be derived, e.g., anaclitic vs. aggressive (or defensive) process, role playing or other behavioral phenomena, and motivation for the development of identification as residing in the relationship between the same-sex parent and child. Mussen & Distler (138), studying "developmental" (anaclitic), defensive, and role-playing hypotheses regarding identification in boys, found that although all three hypotheses were supported, the strongest relationship was between identification, as measured by a sex-role

preference scale, and perception of the father as a powerful source of both reward and punishment. In a later publication using mothers' reports, Mus-sen & Distler (139) found that sex-typing is related to affectionate relationship with the father, that conscience development is related to the total family climate, and that sex-typing and conscience development are related. Although both these studies have the serious limitations imposed by a small, middle-class sample and by possible interview distortions, they are valuable in light of the theoretical trends noted.

In a study of behaving-like-others-in-fantasy, which seems to approximate Bronfenbrenner's "behavior" aspect, Emmerich's (50) most intriguing finding is that, as they grow up, girls seem to be learning how not to be babies, and boys how not to be girls. Rosenberg & Sutton-Smith's (165) study of differences in play between boys and girls lends support to Emmerich inasmuch as they found an increasing masculinity of the girls' self-concept but a clear and definitely nonfeminine self-concept of boys.

*Sex-role studies.*—For adult sex roles, Hartley (80) finds that children's concepts are clearly differentiated, so that although forms of activities appropriate to each sex may have changed in recent years, the functions have not. Hartley & Klein (81) find that children have very explicit ideas of appropriate behaviors for men and women, even when their mothers work. Mothers are still perceived as homemakers, and fathers as money getters, although each may "help" the other. Also, Hartley (79) proposes some interesting hypotheses about the possible sex-role adjustment patterns boys may adopt in relation to adult social pressures for masculinity.

If imitation is one of the bases for sex-role practice, as Maccoby's (123) integration of the theory of Piaget and of Miller and Dollard suggests, then McDavid's (130) finding that girls learn to imitate adult leaders significantly more than do boys may help to account for Emmerich's results reported above.

*Other studies.*—Unlike an earlier study of "Shirley's babies" which took account of patterns, Tuddenham's (191) follow-up of 72 California adolescents, now in their thirties, compared ratings on specific traits. On the whole, correlations were positive but low, certain traits (aggression, spontaneity, expressiveness) being more stable than others. Two predictive studies by Anderson & Harris (2) and Escalona & Heider (51) show that although predictions better than chance can be made over a time span of five or seven years, these are generally restricted to culturally defined behavior patterns and are extremely poor in predicting internal psychological functions and content.

Suggestibility, particularly in relation to adults, was found to be greater in "high" than in "low" dependent children (93). Bronson (21) derived from Erikson's concept of "identity diffusion" in late adolescence four salient characteristics. These were, as predicted, intercorrelated. Validity

is suggested: "Ss who would be considered low on identity diffusion impressed interview raters as having a stable sense of self...."

#### LEARNING

Research in learning is still focused primarily on testing theoretical postulates rather than on studying the phenomenon as it can be observed in human functioning, though Harlow's (76) research on maturation of learning ability in rhesus monkeys attacks one aspect of the realities in learning. But as he points out, the abilities of monkeys are "probably comparable to those of low-level human imbeciles," and can give us no information concerning human language and scant information when thinking is involved.

Studies, such as those of Stevenson & Weir (185), Minnich & London (134), O'Connor & Hermelin (142), and Koch & Meyer (104), by considering such variables as chronological age, anxiety level, and mental age, acknowledge to some extent the peculiarly human attributes of their subjects. Possibly the most intriguing study is that of Clark, Lansford & Dallenbach (30). Their results support Rock's finding that a single trial is better than repetition for the formation and retention of association between letter-number pairs by college students. These results need to be tested on younger people, preferably over a wide range of ages, to discover their developmental implications.

The study of Lipsitt, Castaneda & Kemble (114) shows that differential delayed reward facilitates learning in a transfer discrimination task. Response shift, studied by Stevenson & Weir (184) and Kendler & Kendler (102), and some studies of conditions antecedent to learning such as those by Owen (145), Kanfer & Karas (99), and Steigman & Stevenson (183) represent excellent examples of careful experimentation using children as subjects in order to test learning-theory principles derived from, or substantiated by, animal work. Their results support these principles, and this is, undoubtedly, their justifiable concern.

There have also been several studies on the relationship between personality and learning or learning problems. Norman & Daley (140) showed poorer adjustment and greater rejection of self and greater rejection by others among poor readers. A clinical study (169) finds that learning disability in children with adequate intelligence is associated with other disturbances, all traceable to disturbed mother-child relationships. To learn would be the final surrender of individuality.

A significant and skillful longitudinal study [d'Heurle, Mellinger & Haggard (47)] of personality patterns in gifted children reveals important relationships to achievement in general, along with remarkably specific personality characteristics for those with each of a number of specialized abilities (e.g., generally high achievers show high integration, control, and inner

strain; high reading achievers, tension and free-floating anxiety; high arithmetic achievers, spontaneity, aggression, self-confidence).

Goldworth's study (67) of a part-time special grouping arrangement for fast learners showed that the plan did not affect the stability of the children's social relations.

#### COGNITIVE DEVELOPMENT

The increased interest in cognitive aspects of development, noted by Martin (126) and others, is in evidence in the studies we have placed under this heading and in many studies we have otherwise classified.

*Speech and language.*—Continuing their studies of children's acquisition of language, Brown & Berko (24), using word association and usage methods, found that the tendency to respond with the same part of speech increases regularly with age, presumably reflecting the child's gradual syntactic organization of his vocabulary. Another association study [Davidson & Longo (43)] showed decrease in association time with age in the middle and adolescent years. Heterogeneity of response is as great for young as for older Ss.

In the area of the development of infantile speech sounds Winitz (195) questions Lynip's rejection of earlier work in the field and his claim that only electronically recorded data are "objective." Evidence is offered that sound spectrographs also require interpretation; they can supplement but not replace other means of observation.

An interview study [Goda & Smith (65)] establishes mothers' conviction that conversation and reading aloud are most stimulating to their preschool children's language development, but reveals that in fact (alas!) such activities occupy far fewer waking hours than television, whose stimulation value was rated low. A British study by Sampson (171) showed only moderate positive correlation between developmental language ratings at the ages of 21 months and at five years.

*Perception.*—Pastore continues to present evidence (146) for unlearned perceptual functioning (here, form discrimination) in the duckling, and in a theoretical article (147) he suggests that in origin all significant aspects of perception are unlearned, though considerable modification is possible through experience. Much research confusion of maturation and learning, he suggests, arises from the failure to distinguish between origin and modification.

A study [Leibowitz & Hartman (110)] of the moon illusion showed it to be greater in children than in adults, but the authors' interpretation of the findings in terms of greater adult experience with the viewing of overhead objects remains a matter of controversy [Cohen (36); Leibowitz & Hartman (111)].

Except for discrepancies in age placement, essential confirmation is re-

ported [Peel (148); Lovell (119)] of Piaget & Inhelder's findings on haptic perception and on children's representations of space.

*Moral judgment.*—Although Piaget has gone on to many other concerns, his work of the twenties, particularly on "moral judgment," is still—or again—attracting criticism [Bloom (16)]. Failures to replicate have been reported [Durkin (49); Medinnus (131)] when some conditions have been changed or when social and cultural variables are taken into account, or, when there has been confirmation of his stages, doubt has been expressed regarding the ages Piaget has assigned them [Peel (148)]. Essentially similar material has appeared before.

*Children's thinking.*—Here again the dominant role of Piaget's contributions is evident, and more often than not they provide the point of departure. Braine (18) confirms some of Piaget's findings regarding logical operations, but, by the use of nonverbal methods and the elimination of other alleged confounding factors, shows their presence two years earlier in children's thinking than Piaget found them. Braine's chief purpose is to investigate Piaget's conceptions of intelligence in terms of what he considers more appropriate research design and to confront relevant variables Piaget is said to overlook. Another study, of causality [Mogar (135)], also places lower than does Piaget the age at which inductive reasoning appears. Classificatory concepts in an experimental grouping task [Annett (3)] changed little with age, but the explanations for them did.

Unlike adults or eleven-year-olds, six- and nine-year-olds tend to alternate predictions for a series of random binary events, possibly because of their notions about "taking turns" [Gratch (69)]. Fisher & Fisher (55) report another of their studies of body-image correlates, presenting further evidence for a relationship between attitudes toward body sectors and the sectors' reactivity (here, in terms of sidedness of galvanic skin response gradients). Among other developmental findings, "left directional reactivity is rare until adolescence . . . [when] . . . the incidence . . . rises to approximately 40 per cent."

Richardson & Church (161) report a new scheme for classifying along four developmental dimensions children's attempts to interpret proverbs. Straight-line improvement with age does not occur, since older Ss, trying to take account of metaphor, which younger Ss do not recognize, may revert anew to primitive mechanisms.

#### DISTURBANCES OF DEVELOPMENT

*Parent-child relationships.*—The year has witnessed a continuation of the systematic closing in on significant parent-child relationships and their outcomes in children's pathology. Many of the studies here represent empirical research on clinical hypotheses. Rosenthal *et al.* (167), for example, find a number of highly dependable relationships between various patterns

of maternal behavior and children's problems in a large clinical population. Many of these relationships constitute corroboration of established psychoanalytic views, others are novel. Although the dovetailing of judgments of mothers and children was aided by the community of views of the participating analytically oriented psychiatrists, most of the variables used appear to be sufficiently specifiable to allow replication by others.

Garner & Wenar (60) present a carefully designed study of 26 clinical trios of physically ill, neurotic, and psychosomatically ill school-age children. Mothers of the first group had positive attitudes to pregnancy and early child care, mothers of the neurotic group had rather negative attitudes to both, and mothers of the psychosomatic group felt positive toward pregnancy but not toward infant care. The latter were seen as "being striving and controlling in relation to their infant." The children's attitudes towards their mothers are of a corresponding sort. Several new measures for tapping relevant feelings were developed in the course of the research.

Bandura & Walters (5), excluding delinquency area, cultural, organic, and psychopathological factors, found that 26 very aggressive adolescents differed from their matched controls particularly in disruption of paternal relations. They were far more critical and resentful of their fathers and showed little identification with them.

Among other studies of specific groups of disturbed children, it is reported that the parents of asthmatic children were not significantly different from a control group [Fitzelle (56)] and that 50 mothers of delinquent children differ from their controls on nine of the 20 PARI scales, in general showing more primitive, more controlling, and more authoritarian attitudes [Madoff (124)]. (It is noteworthy that findings like the latter, though obtained rather consistently, have had little or no impact on the awareness of our courts.) A clinical report from Britain by Gluck & Wrenn (64) indicates that disturbed mothers who improve in treatment are often confronted by children who cannot accept the change; treatment procedures for both are presented. Children, presumably predisposed, are said by Greene (71) to develop manifest leukemia when they suffer the major emotional deprivation of being dropped for a substitute after having been the objects of unusual maternal attachment.

Studies of adult and of child schizophrenics continue to yield conflicting data on the significance of parental attitudes. While some contributors [Kaufman *et al.* (100); Lane & Singer (107); Lyketsos (121)] find various rather specific patterns of disturbance in parent-child relations, others [Esman, Kohn & Nyman (52)] find no common pattern but always some severe intrafamilial disturbance. Still another [Klebanoff (103)] finds the mothers of schizophrenic and organically ill children exhibiting similarly pathological attitudes, implying that these are the reactive result rather than the source of the children's disturbances. Haggerty's data (74) show language impairment and lowered grasp of reality in long-hospitalized children.



He suggests that such impairment may contribute to, and not merely reflect, a schizophrenic process.

Another study [Becker *et al.* (9)] of pathogenic parental attitudes, this time with "problem behavior" as the dependent variable, revealed meaningful patterns of relationship between two patterns of children's disturbance and maternal and paternal attitudes. The present adjustment of a group of institutionalized adolescents was related [Sklarew (179)] to early separations of at least one month from either mother or father; separation in the first three years was most damaging in producing severe nonconforming behavior—but this in boys only; for girls the unexpected correlate of early separation was superior adjustment rating.

*Perinatal disturbances.*—Stott (187), following up as school children a group of British infants who had been early hospitalized for two weeks or more, found a higher incidence than among controls of reading disturbances and "unforthcoming" personality. He posits a common congenital origin for the illnesses, retardation, and personality effect. A more direct study of minimal diffuse cerebral insult by Kawi & Pasamanick (101) dealt with 372 boys with severe reading disorders. For 205 of these, obstetrical histories were obtained and compared with matched controls. Children with disorders showed significantly greater incidence of obstetric complications; the more severe the reading difficulties, the higher the incidence of such complications. Most prominent were toxemias and bleeding during pregnancy.

Cobrinik (35) found severity of motor impairment in brain-injured children (and hence degree of neurological damage?) to be related to difficulty of hidden-figure perception. Scarpelli (172) induced in pregnant rats thiamine and iron deficiencies of sufficient magnitude to be demonstrable in the offspring but failed to find any effects on the maze-running ability of the young.

*Other studies.*—An unusual demonstration of the refinements of theory and of design of which clinical research is capable comes in a study of sleep disturbances made by Anthony (4), holder of the first American chair of child psychiatry, while still at Maudsley. Disturbed sleepers were found more likely than a (neurotic) control group to have experienced maternal separation, to have phobic mothers, to have (along with their mothers) more sex fears, more tension and fearfulness, more suggestibility. Reliable differences between sleepwalkers and those with nightmares were also found, particularly in strength of eidetic imagery. Sleepwalkers were low on WISC verbal scores, being generally inarticulate. Electroencephalogram findings are also reported. Over-all, the findings lead to a discussion of symptom choice which constitutes a remarkable synthesis of psychoanalytic concepts and those arising from consideration of the whole system of organismic thresholds and sensitivities.

Another Maudsley study [Bene (10)] may be regarded primarily as a

demonstration of the recently developed Family Relation Test (a sort of MMPI-like card-sorting procedure, with acts and attitudes to be assigned to dolls representing the family) as a method for getting at children's views of the feelings they impute (openly, not projectively) to the important persons in their lives.

Siller (177) reports good adjustment on amputated children and their parents to the handicap; however, his findings seem open to reinterpretation.

Studies of retarded children show that if problems are sufficiently simple, they are capable of forming learning sets [House & Zeaman (90)] and that Mongoloid children cannot solve discrimination problems as well as normals of similar mental age [Girardeau (63)].

A promising approach to delinquency uses factor analysis to identify distinct personality and background factors as a basis for future research [Peterson, Quay & Cameron (149)].

*Therapy.*—A British study [Lunzer (120)] following selected groups of aggressive and withdrawing children should be kept in mind in future evaluations of therapy; about half of each group showed spontaneous improvement.

Although an untreated control group in which spontaneous improvement could be observed was lacking in Cytryn, Gilbert & Eisenberg's investigation (39) of tranquilizers, widely spaced therapy sessions for disturbed children and their parents were equally effective in producing improvement in some types of cases whether accompanied by a placebo or by either of two drugs tested.

Sortini (182) clearly confirms the importance for children with hearing loss of very early use of hearing aids. Evidence on this point appears, recently, to be converging from many sources. Phillips & Haring (152) report that emotionally disturbed children do better in "structured" than in "permissive" schoolrooms. Here and elsewhere, the lesson of Lewin and his collaborators that such "atmospheres" cannot be meaningfully discussed in terms of a simple dichotomy appears to have been forgotten. The usefulness of special speech therapy for retarded children is asserted by Schlinger (174) on the basis of a longitudinal study of 12 children.

#### GENERAL CONTRIBUTIONS

Another well-known text [Jersild (94)] in child psychology has appeared in revised form this year and has been joined by a new text [Hutt & Gibby (91)] and a new book of readings [Haimowitz & Haimowitz (75)]. A well-established text on adolescence [Cole (37)] has also appeared in a revision. Fraiberg (58) offers an insightful, rather popular treatment of development before school age based on modern psychoanalytic views. A thorough compilation of British children's games, language, magic, and the like [Opie & Opie (144)] is now available. Texts on child psychotherapy [Moustakas (137)], child clinical psychology [Ross (168)],



and on "psychoeducational therapy" [Berkowitz & Rothman (11)] have also appeared, emphasizing the developing specialty of therapy for children.

Brim (19) offers a review of the history and the assumptions of parent education; his evaluation of it in operation is inconclusive, and he finds that it is not possible on the basis of present information to estimate the effectiveness of such education.

Potentially, at least, one of the most significant publishing events of the year was the appearance in England of Volume 1, No. 1 of the *Journal of Child Psychology and Psychiatry and Allied Disciplines* (96), intended as an international, interdisciplinary journal which will combine "those approaches that study the child as an individual organism with those that view him as a developing social being. . ."

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# COMPARATIVE PSYCHOLOGY<sup>1,2</sup>

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## INTRODUCTION

The present author has encountered the same difficulty as all his recent predecessors in deciding which parts of the welcome, though undeniably oppressive, mass of recent literature on animal behavior should be selected for review. In making the choices of the fields to be covered he has been guided by three main principles: (a) the theoretical importance of the field, (b) the year in which it was last covered in this series, (c) the amount of relevant published work. Where there is more than one paper covering a particular field he has tended—other things being equal—to prefer that paper which best summarizes and covers the recent references.

Some reference to ethology as a discipline apart from comparative psychology, yet of vital importance to it, has occurred in at least the last five contributions under this heading. This fact is itself eloquent testimony to the importance which the topic is generally believed to possess, but it is quite clear that the final word has not yet been spoken.

The word ethology in English originally meant "the interpretation of character by the study of gesture." Some 35 or more years ago it came into general use among zoologists to signify the comparative study and analysis of the instinctive or stereotyped movements of animals. It came to mean, in fact, the comparative anatomy of gesture of animal species. This zoological use was actually a narrowing from a wider scientific use prevalent in the early years of the century, when the term ethology was sometimes used to mean simply the scientific study of animal behavior. The present writer is convinced that this wide use is the right, indeed the only logical, use for today, and the term as so defined has great convenience for the zoologist as a complement to the term ecology. A valuable general introduction to the background and a critical study of a great deal of the recent literature have been given by Hinde (39).

It is obvious that ethology defined in this way includes comparative psychology, since both comprise the objective scientific study of animal behavior. There is, however, a rather fundamental difference of approach between the people who call themselves comparative psychologists and those who first adopted the designation ethologists. The psychologist is primarily interested in mechanism and motivation in the individual and its significance

<sup>1</sup> The survey of the literature pertaining to this review was concluded in May, 1960.

<sup>2</sup> This review is dedicated to Professor Otto Koehler on the occasion of his 70th birthday.

for general theories of behavior. Even though he may study animals, he is usually a person who has been trained in human psychology and physiology. On the other hand, the original ethologists, indeed most of the modern European workers in this field, are zoologists by inclination and training. Their preferences and proclivities arise from this background. The zoologist is interested in the comparison of species with a view to understanding how behavior evolved, how it became stereotyped here and flexible there—in short, he is still very largely dominated by the concepts of evolution and by the desire to trace origins and phylogenies. For this reason no zoologist studying the behavior of animals can fail to be struck by the uncanny similarity in the fixed or “instinctive” components of behavior which related species exhibit. Accordingly, the ethologist in the restricted sense is apt to select the animals that he studies from among a wide range of natural species, in contrast to the rather narrow range adopted by the psychologist who seldom deals with species other than rat, monkey, pigeon, ape, and man. Whenever psychologists and ethologists seem to be at cross purposes in discussion it is always worth-while to look again at the fundamental method of approach, for in this way the misunderstanding or conflict often can be readily resolved. Disagreement frequently resolves itself not into a question of disputed fact but of intellectual attitude. From the zoologist's point of view, the coming of ethology was a reaction against the uncritical anecdotalism of much of the writing on animal behavior of the last century. With its emphasis on objectivity, ethology represents the scientific rebirth of one major part of natural history [see Thorpe (91)]. Indeed, we are all ethologists in the new sense. The old division is useless, gives rise to frustrations and misunderstandings, and should be dropped, and this regular chapter in the *Annual Review of Psychology* should, in the writer's opinion, be renamed “Ethology.”

Although ethologists pride themselves on their objectivity, it is still not uncommon for so-called comparative psychologists to upbraid them for giving too much weight to observations in the field, sometimes of a natural-history character, which the laboratory experimenter on learning, for instance, may feel to be beneath the notice of one who claims to be prosecuting an exact science. The writer believes there is an important point of difference here. We all want greater precision and better design of experiment; but psychology is not physics, and if we pretend that the methods of physics, and only the methods of physics, are adequate for the study of behavior we shall miss much of vital importance to the subject. Indeed, to restrict ourselves completely in this respect would be obviously stultifying. To take one recent example, the book *Born Free* (1) gives a popular account of the hand-rearing of a lioness to full breeding maturity in the wild, which engaged the greater part of the time and attention of one person for nearly three years. The account is obviously anthropomorphic, subjective, and, from the nature of the case, “anecdotal” and for these reasons might be

considered to be beneath the notice of an experimental scientist; nevertheless, facts of striking ethological interest emerge. Until the hand-rearing of young lions to maturity becomes an accepted research technique, sponsored by one of the foundations and carried out on a scale large enough to give results amenable to statistical treatment (a prospect which we can be sure is very remote indeed), it would be folly to ignore the significant facts in a study such as this.

#### ANALYSIS OF INSTINCTIVE BEHAVIOR

The zoological pioneers in the ethological approach to animal behavior have always been particularly attracted—partly because of their comparative evolutionary approach—to the relatively stereotyped and inflexible species-specific behavior of animals generally known as “instinctive.” To them this type of behavior forms the most attractive subject for analysis, and they find almost incomprehensible the reluctance of the comparative psychologists of the early years of this century to take instinctive behavior and its theoretical implications seriously (95). Consequently this field, which is almost limitless, remains and will remain for a long time one of the major research areas. At a recent ethological conference attended by nearly 150 research workers from many different countries, by far the largest single group of communications (constituting about half the total) dealt with analytical and comparative studies of instinctive behavior. The contributions in this field are so numerous that only a few representative studies can be mentioned in this chapter.

The vexing question of the theoretical definition of instinctive behavior to decide whether we are concerned in any given case with an instinctive pattern of behavior or with a learned one is to consider the origin of complexity. Where does the complexity of the behavior come from? If we can see the necessary complexity in the input from the environment that is being or has previously been experienced, then we are provisionally justified in assuming that it has been learned. If, however, there is complexity in the behavior pattern that is not seen in the immediate, or in the whole previous, experience of the individual animal, and that cannot be merely the expression of bodily structure, then we have to assume that this complexity comes from somewhere else, and that it can have come only from the inborn organization of the animal. To take an obvious example, if a whitethroat or a garden warbler, hand-reared from the egg in a soundproof room and maintained under a constant and simple regime of physiological stimulation, comes in due course to produce the perfectly characteristic and exceedingly elaborate song of its own species, then we can say quite definitely that there is no system in the environment to which it has been exposed that can “account for” the complexity of this behavior pattern of singing. Therefore we conclude that the pattern is innate in the sense that it must be in some

way coded genetically and in the central nervous system and that it is not even dependent on auditory feedback—as has been shown by operatively-deafened birds. On the systematic and evolutionary aspect of instinctive behavior there are, in the period under review, two important review papers by Tinbergen (97, 98).

Another excellent recent example of the intensive comparative study of instinct, the work of Crane (18, 19) on the display movements of the fiddler crabs of the genus *Uca*, may be mentioned. This genus is distributed throughout the tropics. There are, among its 50 or more species, some that are narrowly restricted in distribution, others that are less so and found in numerous isolated populations on tropical islands and shores, often separated by thousands of miles of open ocean. The sexual and aggressive displays consist of conspicuous movements of the single large chela, and for nearly every species these movements are distinctive in both amplitude and movement pattern. They constitute, in fact, specific characters as reliable as any that the morphologist can find. Thus, populations of the same species in most widely separated localities will show, under similar conditions and provided that they are sexually mature, identical display gestures. However, many of the species will, while still immature, produce gestures that have not yet attained the specific norm but are, in fact, similar to, if not identical with, the gestures of adult crabs of other species. Here we have a series of actions that are of extreme fixity when the crab is mature—actions that are neither reflexes nor, in any ordinary sense of the term, learned. Such results raise, in an acute form, the problem of "typical intensity" (76). It is necessary not merely that the form of the movement should be coded in the nervous system, but that the timing and intensity should be under equally rigid control. For the maintenance of the specific constancy of the fiddling movements of *Uca* there must be a neurohumoral mechanism of almost unbelievable precision in overall control. Such a situation provides a striking challenge to the invertebrate endocrinologist.

Dixon (23) reports remarkably complex avoidance and protective responses (apparently visual) in aphids on seeing the predatory ladybird, *Adalia decempunctata*. The aphid walks away and drops off the plant or kicks. If caught, it waxes the head of its enemy with its cornicles, causing temporary paralysis during which it can often escape. This behavior calls for full analysis, since learning cannot be involved.

In view of past controversies, the increasing sophistication that has been evident during the last two years in reports published on the analysis of instinctive behavior is exceedingly welcome. A great deal of new light has been thrown on the study of instinct, as it is hoped this review will show, and as a result many of the old objections to the term "instinctive" no longer stand. One of the earlier authors in this series (99) was bold enough to say that "instinct is once again dead." I think few ethologists would have agreed with that view at the time it was expressed; all one can

say is that if it was true then, there has been, within two years, a remarkable resurrection. A few examples of these increasingly sophisticated analyses follow.

Blest (10, 11, 12, 13) has analyzed the curious rhythmic settling movement, called rocking, of Saturniid and other moths when assuming the resting position from any preceding activity. By a series of extremely elegant physiological techniques, this movement is shown to be dependent on a number of factors such as the age of the animal, the presence of competing reproductive responses, and, above all, the duration of previous flight performance. It is shown that the strength of the rocking response is mediated by central nervous interactions and that no afferent input other than that required for the release of the flight response, and subsequently of the settling response, is required. Blest plausibly suggests that the mechanism which is found in the thoracic centers of the Hemileucine moths is such as could account for the relationship found in the honeybee between the distance flown from the hive to the food sources and the strength of the rhythmic waggle phase of the communication dance. He refers these and other related behavior patterns to the general problem of the concept of ritualization, to which he contributes some important new ideas (14).

Dethier & Bodenstein (22) and Dethier (21) have shown that the feeding drive and food choice mechanism in the blowfly can be accounted for without any reference to a theory of centrally controlled unitary hunger drive. The elaborate series of movements resulting in the extension of the proboscis to the feeding position may be brought about by the excitation of a single taste receptor cell, and the degree of preference exhibited depends on the alternatives present. If the choice situation involves odorous compounds, simultaneous comparison of stimulus intensity is made. If taste alone is involved, the choice is in fact random, the amount taken on the first try depending solely on the intensity of the stimulus and the development of peripheral adaptation. The amount taken on the second and subsequent visits depends on the duration of peripheral activation and the effect of post-digestion factors arising from the previous visit. Neither in the case of odorous or nonodorous foods does memory or learning appear to be important, and the whole pattern of feeding behavior can be plausibly accounted for on the basis of receptor analysis. In matters of food choice, the blowfly is rigidly stimulus-bound.

A somewhat similar picture emerges from the experimental analysis by Kennedy (52) of the flight and other behavior of aphids. He finds that there are central nervous factors governing responses in these insects as well as immediate stimuli from outside, but he believes that these internal factors are themselves part of and "can be studied only through" reflex activity in the "comprehensive physiological sense of the term" and that they do not belong to a separate category of facts. It is not surprising that in birds, in contrast to many examples in insects, differential preferences

are not readily attributable to the receptors, but must be presumed to be due to more centrally situated mechanisms. Another noteworthy contribution to the study of instinctive behavior is Morris' analysis of the reproductive behavior of the ten-spined stickleback (77).

There have been numerous studies on the inheritance of behavior traits in mammals where behavior is exceedingly complex and inheritance, presumably, mainly polygenic. Perhaps the situation in insects, being simpler, offers better prospects for fundamental analysis. Two recent examples seem particularly worthy of mention. Haskyns & Haskyns (32) studied the food habits and cocoon-spinning behavior of moths of the genus *Callosamia*. They found that the  $F_1$  hybrids resembled the male parent in food habits as well as in morphology, but that in cocoon-spinning there was clear evidence that the inheritance of the behavior pattern was polygenic in character. Manning (68), in a study of the courtship behavior of *Drosophila melanogaster* and *Drosophila simulans*, concluded that changes in courtship behavior by which these species can be characterized could most readily be produced by the selection of mutations affecting reaction thresholds and that these would be a first step in behavioral divergence. He has further shown experimentally (69) that artificial selection for speed of mating in *D. melanogaster* will change the threshold of the various more purely sexual responses.

Among other straightforward analyses of the behavior of higher animals, Dane, Walcott & Drury (20) have analyzed display actions of the goldeneye duck. Some movements were found to be exceedingly constant in both form and duration, others constant in form but variable in duration, and a few variable in both. Walther (105) has published a very extensive analysis of the fighting and mating behavior of a series of antelope species.

Finally, it may not be out of place to mention descriptions of two examples of what appears to be innate behavior in human beings. Prechtl (82) has made a quantitative and qualitative analysis of the nervous mechanisms of the side to side movement and directed head-turning movement of human babies when first seeking the breast. The movement may occur either in response to tactile stimulation near the mouth or, in hungry newborn babies, also in the absence of such stimuli. Two or three weeks after birth the rhythmical movement develops into a spatially oriented movement, and in due course a fully oriented turning of the head towards the stimulus—a directed head-turning—takes the place of the original rhythmical movement. In premature babies rhythmical searching persists considerably longer. Wolf (112) has studied the behavior of newborn male infants in sleep and has shown that the startle reflex and incipient behavior components of other responses occur regularly in the absence of external stimulation.

The examples of instinctive behavior so far discussed are relatively stereotyped and do not show much evidence of adjustment by learning and experience. No one will deny that instincts are often self-differentiating



and that learning often plays a vital part in their development. It is now almost universally recognized among ethologists that the more complex action patterns which would previously have been labelled instinctive almost invariably include important learned components [see Hinde (39, 40, 43)]. The comparative psychologists can justly claim a large part of the credit for this development in the climate of opinion. The problem that is now the subject of vigorous discussion is how big and how complex are the strictly innate components of instinctive acts. How large are the "chunks" of instinct, and does learning infiltrate right through the instinctive acts or not—that is to say, are learning and instinct miscible in all proportions?

Work in recent years on priming, imprinting, and one-trial learning, much of which has been discussed in recent contributions to the *Annual Review* and which, consequently, will be little dealt with here, has shown how it comes about that an innate organization can predispose or, indeed, compel an animal to learn in a particular direction and at a particular time. A striking new example of this comes from the work of Rosenblatt & Aronson (86) and Aronson & Rosenblatt (3). It was found that when comparing normal with hormonally induced sexual behavior in male cats, one full experience of successful copulation is all that is required for later success. The effects of early deprivation of experience persisted in spite of later hormone treatment and experience. Thus, in this instance, as in several other studies, it appears that a major contribution of androgens is that they help to provide an effective morphological and physiological milieu for the development of sexual behavior patterns through experience. A related study is that of Larsson (59) working with rats. Continuous changes in the sexual behavior of male rats with age were noticed. On comparing the performance of experienced with nonexperienced rats of the same age, it was found that only one out of the four characteristics measured, namely the post-ejaculatory latency, was affected by experience. The other changes appearing in the pattern of behavior with increasing age were dependent on maturation alone. Among later studies in this field are (9, 83, 114).

The reproductive behavior of two species of birds of very different types is the subject of detailed and long-continued analytical investigation, some of which has already appeared in print, with the publication of much more impending. Hinde (38) and Hinde & Warren (44, 106) have studied the nest-building behavior of naive canaries. One series of experiments involved the provision of abundant nest material during 30-minute watch periods, the material being removed at the end of the period. The three main behavior patterns concerned with nest building are gathering material, carrying material, and sitting building. Contrary to what might have been expected, sitting building is not consummatory to gathering and carrying; rather all three seem to be under the control of a single causal factor—presumably hormonal—that determines the threshold at which they can be elicited. But each activity can interrupt the other. The only scheme



which seems to fit the facts is to suppose that the activities share the motivation but that each, independently, has some inertia of its own and that each is building up some inhibition as it proceeds. In other words, the actions are both self-stimulating and self-exhausting, to quote a phrase coined by Julian Huxley for a description of the great crested grebe, published in 1912. The reproductive cycle in birds depends, of course, primarily on internal changes set in motion by external factors such as changes in day length, temperature, etc., but the successive phases of the cycle do not then follow automatically but depend on further stimuli from the environment or further internal changes or both. Hinde & Warren find that the female may lay eggs without going through all the earlier phases of reproductive behavior but egg laying is then delayed and clutch size is abnormal. For biologically successful reproductive behavior, nest building activities and a complete nest are essential. Innate recognitions undoubtedly come into this. The nest itself, stimuli from the nest pan, the size and texture of the nest material, recognition of grass and feathers, and so on, and learned responses are clearly also important.

The impressive work of Lehrman (60, 61) on the organization of the breeding cycle of the ring dove, *Streptopelia risoria*, has proceeded much further since the last review in this series. Lehrman has summarized the whole field of the hormonal responses to external stimuli in relation to breeding biology and physiology generally in birds (62). It appears that a number of movement patterns characteristic of incubating doves are induced in inexperienced birds by progesterone injection. These movements are independent of experience but combined into effective behavior, at least partly, because orientation to the egg has to be acquired by experience and is lacking in the naive birds. Lehrman concludes that in many species of birds the stimuli provided by nest material, nests, and perhaps the sight of others building play an important regulating role, and in some cases an essential causative role, in the progression to the next stage in the physiological cycle. And there is ample evidence that different pituitary hormones can be selectively stimulated by different stimuli acting through the nervous system. Because naive female canaries kept without males will often build nests and doves kept in isolation will occasionally lay eggs, it would be going too far to say that all the endocrine changes occurring during the breeding season are caused solely by the effects of external stimulation; but—to quote Lehrman—

it is surely permissible to say that in very many species of birds external stimuli provide at least a sufficient regulated influence on endocrine secretion to be one of the causes of the delicate adjustment of successive changes in the bird's behaviour to successive changes in external stimulation.

Wood-Gush (113), comparing the sexual behavior of brown leghorn cocks

reared in isolation with controls, found no clear-cut difference in the behavior of the two toward females.

The modern experimental work on the development of song in birds is a behavioral attack on the same essential problem. The following recent references are relevant: Hinde (37), Marler (70), Poulsen (81), and Thorpe (92, 93, 94).

In the chaffinch, the hand-reared isolated bird produces extremely simple songs which represent the inborn component of the specific song. The completely isolated bird can do no more than this. But this completely innate song is almost identical with the normal song of a neighboring species, the blue chaffinch (*Fringilla teydea*) of Tenerife. If a hand-reared chaffinch is allowed even the simple auditory stimulus provided by a singing competitor as inexperienced as itself, this will help it toward normality, at least in achieving a division of the  $2\frac{1}{2}$ -second song into its normal triple form with a stepwise descent of frequency. It seems, then, that while the innate tendency to produce the true Kaspar Hauser song is strong enough to govern the bird's behavior even in complete isolation, there are other parts of the inborn pattern which, although genetically coded in the same way, need the stimulus of competitive singing to enable them to emerge into the actuality of performance. The full details of what can be considered as the specific song and also the innumerable finer peculiarities of performance by which the individual can be recognized can be acquired both in the wild and under experimental conditions as a result of competitive singing with neighbors. There is a period of about 13 months (up to the middle of the first breeding season) that appears to be a period of particular sensitivity to auditory stimulation and during which, and only during which, the chaffinch can elaborate its full song from the innate material in the subsong and from what it hears from other individuals. The basis of the probably innate restriction of its limitation to songs of members of its own species has been discussed by the present writer (92, 93). Poulsen (81) arrives at very similar conclusions from his study of the song learning of the domestic canary. He finds the characteristic song of the roller canary genetically fixed and able to develop without any chance of imitation. Much valuable information and a good discussion are provided by Marler (70) in relation to the wider field of animal communication.

#### NEUROPHYSIOLOGY AND DRIVE IN INSTINCT

Only a few years ago it could still be argued with some degree of plausibility that there were serious neurophysiological difficulties in the view that instinctive drives could be truly endogenous in the sense of arising within the central nervous system independent of peripheral stimulation. Indeed, some writers [e.g., Kennedy (52)] still seem to be in this quite unwarranted state of mind. Modern developments in neurophysiology are,

however, fast changing the climate of opinion in regard to drives and the motivation of elaborate innate behavior patterns that they control. In the past the drive concept has been grossly overworked and gratuitously invoked, and the uncritical employment of the concept has done great harm to comparative psychology. Drive concepts tend to acquire a unitary character and to be regarded as unanalyzable entities in a causal network. The critical discussions of Hinde (41, 42) help to restore the balance. Drive concepts are useful, indeed essential, at some levels; at others they may be misleading. But it is from direct neurophysiological work that the greatest advances are coming. Bullock (16) has expressed the conviction that neural discharges are probably normally patterned—temporally and spatially distributed—in a meaningful and nonrandom way. He says "One way of stating the function of the nervous system, or of any significant part of it, is that it formulates appropriately patterned messages in code." The purely temporal aspect of the neurophysiology of innate behavior patterns has been greatly illuminated in recent years by the discovery of pacemaker cells and even subcellular mechanisms, some of them temperature-independent or temperature-compensated, which act as fixed-frequency alarm clocks setting off behavior patterns at regular intervals. The particular case of circadian rhythms is discussed below. The whole subject has been reviewed recently by Aschoff (4), Harker (28, 29), and Thorpe (96); other important references are those by Pittendrigh & Bruce (79, 80).

But the evidence from the study of endogenous biological pacemaking systems, important though it is, is eclipsed, from our point of view, by recent developments in the technique of brain stimulation for the study of the innate behavior of the domestic fowl. The paper of von Holst & von St. Paul (49) is something of a landmark for ethologists. This work was in its turn made possible by the straightforward ethological study of the domestic fowl by Bäumer (8). Only the first preliminary results of this neurophysiological study have so far been published, but it has already been shown that point stimulation by oscillating current at a wide variety of loci in the brain stem elicits a very broad range of both simple and complex movements belonging to many different instincts. These include highly characteristic and ethologically unmistakable actions of care of the body surface, picking and cleaning, orientation in space, directing attack or escape behavior to a particular direction, behavior appropriate to attack on or flight from different kinds of enemies, agonistic behavior, sexual behavior, and the care of the young. There is no evidence as yet that any of the loci touched by the electrodes are, in a morphological sense, unique centers for the behavior elicited; on the contrary, it is often clear that the impulses are conducted to distant and, as yet, unlocated systems which themselves control the behavior.

Among the questions upon which light has been thrown already are those of central adaptation, lowering of threshold, the significance of sub-

threshold excitation, and the summation of subthreshold events to rise above the threshold. Of particular interest is the fact that with increasing intensity of stimulation one can produce from one area many different reactions. Out of this many-sided behavior and the mixtures of behavior which result, four clear conclusions emerge: (a) the order of occurrence of different types of movement is not exchangeable, (b) the same sequence is also obtained with constant medial stimulation, (c) these sequences are spoken of by experts as "natural," and (d) their components all serve one particular function. On this basis it is concluded that the electrical stimulation is activating the same structure as that responsible for the unitary drive, with the different parts—especially their individual thresholds—revealed. Since many single acts are contained in more than one complex system and since many such acts may be elicited at a great many different loci, the present overall picture of the organization of an instinctive center of the central nervous system is very similar to that envisaged in Tinbergen's well-known system—that is, a network which is also in some degree a hierarchy, or a network with intermediate integration levels.

Another remarkable feature is the evidence that this work provides not only for vacuum activities but for behavior which, although occurring *in vacuo*, is more intense if the appropriate object is present. Finally, there are behavior sequences which so far have never been observed *in vacuo*, such as (in a cock) attacking rivals or enemies, or (in a hen) picking the feathers out of a socially inferior hen. These require at least a suitable model of the object of this behavior, and the behavior becomes better, the better the model is. With neural stimulation alone, one sees merely threatening gestures *in vacuo*, not the full behavior pattern. It will no doubt be a long time before the full value of this method can be assessed. The ultimate aim of the work, which may prove unattainable, is the elucidation and description of the total operational system. Von Holst & von St. Paul stress the very important point that functional analysis must proceed independently of the histological analysis of the brain in order to avoid the great danger of rash conclusions about localization. The two paths must proceed separately, and not until they have gone a long way will function be related to structure. But whatever the future of the method, there seems no doubt that it has already provided most graphic illustration of the existence of a central unitary drive system or series of systems in the brainstem involved in the precise control of instinctive movements of many different levels of complexity. The method is already being applied to invertebrate brains by Vowles (102, 103) and others. The study of Olds (78) on the rat is also producing results of the greatest interest for students of behavior. It does not yet, however, have the same clear-cut implications for ethology and cannot be so well interpreted since it has not been preceded or accompanied by the same acute and detailed behavioral analysis.

## THE ANALYSIS OF RELEASERS

Experimental analysis of the receptor side of instinctive behavior—the innate releasive mechanism system—shows the same noteworthy increase of sophistication and precision as that on the effector side. It is part of the general problem of accounting for the control and release of instinctive actions appropriate to very special situations that the animal may never before have experienced in its lifetime and, therefore, has had little or no opportunity to adjust to by learning processes. The response must be immediate and correct if the creature is to survive. The problem is usually thought of in such cases as the response to elaborate visual stimuli as, for instance, those provided by the sexual partner in birds and the immediate instinctive recognition of predators such as hawks and owls. But it is by no means confined to the visual side and there are good examples of innate specific releasers in all sensory modalities. The term releaser denotes a special kind of unconditioned stimulus, usually of a specialized and elaborated nature. A good, recent example is the way in which some night-flying moths will either flee or go into catalepsy in response to the high-frequency calls of bats which hunt them by echo location. Although they have some response to sounds ranging from 10 to 200 kilocycles per second, the strongest response occurs between 40 and 80 kilocycles per second, corresponding with the frequencies used by many species of bats [Griffin & Novick (26)].

The ethological analysis of releasers usually follows two related lines of experimentation: (a) the analysis of the releaser by breaking it down into its constituent sign stimuli, and (b) the search for supernormal releasers. Work along these lines seems to be leading to the general conclusion that very often the releaser system, which we find in nature, is in effect a pattern or combination of patterns which, within the limitations set by the general environment, can provide the most stimulative series of sign stimuli—i.e., can come nearest to the supernormal for the largest number of its constituent stimuli. It has long been known that the pecking response of the young chicks of some species of gulls, by which they induce their parents to feed them, is primarily released by a colored spot on the bill of the parent. It has been shown (109) with the black-headed gull that among a large variety of models tested it is possible to find various shapes, proportions, and dimensions that are better than nature. Thus a longer bill is more attractive to the chick than one of normal size, but, no doubt, mechanical and structural reasons have prevented the black-headed gull from evolving a very long thin bill. Work on the brooding response of the herring gull with its large spotted eggs showed that gulls actually prefer to sit upon eggs that are larger than normal and will even accept and try to incubate eggs so large that brooding is impossible (5, 58). It was also found that eggs which are darker than normal are preferred to normal ones, that eggs which have more spots and darker spots than normal are also preferred. Increase in stimulative efficiency of one component of a releaser can often counter-

balance the decreased efficiency of another sign stimulus which goes to make up the same releaser.

A very elegant study is that of prey recognition in the praying mantis (85) where it has been shown that suboptimal dummies cause complete refractoriness within minutes, while optimal prey stimuli continue to elicit strikes after several hours. It was shown that the strike-releasing qualities of the prey are not learned through experience but the stimulus-response relations of the subsequent act of catching and putting to the mouth can be quantitatively modified during the individual life. Other particularly noteworthy recent examples of releaser analysis are as follows: insect hearing and sound production, Alexander & Moore (2); insect visual responses, Magnus (67); birds, Immelmann (50), Rheingold & Hess (84), Melzack, Pennick & Beckett (72); and mammals, Tembrock (90) and Harlow (30). The result is that the original clear-cut picture of the releasing mechanism as a lock exactly fitted to receive the key of the stimulus situation has come to be doubted. If all or most of the usual constituents of a releaser can be varied and improved upon in experimental situations, there is little left of the innate releasive mechanism concept in its original form.

These and many other recent studies like them render it probable that more than we formerly imagined of the selectivity of the animal for its environment must be due to its receptors, and correspondingly less due to central organization. Instinctive visual responses to those stimuli which solely or primarily comprise color and movement may very well be largely dependent on the structure of the receptor. If, on the other hand, they are indeed concerned with form as distinct from movement (a distinction not very difficult to make in practice), we tend to be forced back on the theory of central control. But even here recent work suggests caution. Lettvin *et al.* (63) found that over a large part of the frog's retina it is not the light-intensity itself but rather the pattern of local variation in intensity that is the factor exciting the fibers of the optic nerve. In fact an immensely greater amount of the analysis of the visual stimulus is going on in the retina than had hitherto been conceived possible.

If similar mechanisms exist in the higher vertebrates, many of the views of ethologists about releasers may need still further reformulation. The idea that the complex stimulus relationships that make up the external releaser as we see it are, before they cause the animal to respond, subjected to a series of physiological filtering devices has recently been developed by Marler (71). There are three types of filtering: that imposed by the receptors, by the receptors' efferent pathways and central nervous system functioning together, and by a central filtering mechanism more or less equivalent to the original concept of the innate releasive mechanism (IRM). In instinctive behavior we are thinking of a filtering mechanism which is built in or self differentiated; in learned behavior we are considering filtering which arises from the individual experience of the animal. It should,



however, be pointed out that while learning may be playing a hitherto unsuspected part in the perception of what may often have been regarded as instinctively coded releasers, there are, nevertheless, innumerable cases in which built-in sensory processes may in fact be playing a far more important role in learning than had previously been thought—even, as Marler shows, in creatures like mammals, where instinctive processes had been thought to make only a minor contribution to behavior.

Broadbent (15) has recently pointed out the profound importance of studies of animal perceptual learning for the understanding of human perception. Even the mammalian nervous system is of too small a capacity to transmit all of the information reaching the sense organs, and, therefore, the incoming information must be filtered before any coding mechanism, which allocates a particular output to each input, comes into play. So there are complex and important filtering mechanisms between the periphery and the center.

Barlow (6) has considered what is essentially the same fundamental problem from the point of view of the physiologist, exploiting the idea that removing redundancy by compressing the sensory message into the smallest possible channel might be the first step in learning to discriminate sensory stimuli, a task which is easier if the sensory information is presented in a compressed form. And so the message compression which takes place between the sensory stimulation provided in the releasing situation and the IRM may be very much the same as that which takes place between sensory experience and discrimination memory in man. This is an instance where the formulations of information theory yield much of promise, and it is suggested that the factors and necessities leading to message compression—leading to the production of a code in the central nervous system—may be the same that lead to the ordering of memories and so may be part of the more elaborate psychological functions underlying what we speak of as “intelligence” in human beings.

#### ORIENTATION

The methods employed by birds and other animals to guide themselves on long-distance migratory and homing journeys still constitute one of the most important and one of the most difficult problems with which students of animal behavior concern themselves. It is now well established that day-flying birds on their normal migrations maintain their direction primarily by orientation in respect to the sun in combination with their own internal time-sense or clock, and there is now very strong evidence that night-flying migrants similarly orient themselves by means of an inborn recognition of the salient star pattern (88, 89), although there are some contradictory results as yet unresolved concerning the effect of clouds in disorienting birds (73).

ASTOUNDING as these results are, the most acute problems concern not



the normal migration flights but the ability to home to the nest territory or other familiar location when artificially displaced in unknown country. The extent of this problem has been graphically re-emphasized in the recently established ability of the Laysan albatross to home in exceedingly short time over vast stretches—up to 1500 miles—of ocean after displacement by airplane (53). Work on this baffling subject has been greatly hindered by the tragic death of Gustav Kramer who was the outstanding worker in this field and who with his pupils and associates had maintained extensive research programs in both Europe and the United States. Kramer's last results (57) emphasize his skepticism as to the possibility of explaining these results by any form of Matthews' sun-orientation hypothesis. Nevertheless, at the time of his death he was convinced that a physical explanation would be found though he was not ready to propose any alternative scheme.

The experiments of Kramer and of Wallraff (104) showed that the homing ability of pigeons was eliminated if the birds were reared in an aviary surrounded by palisades in such a manner that free sight of the sky was not allowed except for a zone of an elevation of three degrees above the horizon. Other recent results, including experiments designed to shift the bird's internal clock, are discussed by Kramer. The ability of albatrosses to orient when released over the open ocean suggests, however, that the interpretation of the palisade experiment leaves something to be desired. The whole matter demands a fresh experimental attack with new ideas. The facts that there are consistently good and bad homing directions when birds are liberated in unknown country and that these directions differ in different geographical localities (e.g., England, Germany, and North Carolina) had been thought of by Kramer as possibly due to selective differences in the stocks of homing pigeons used. This possibility seems to have been eliminated by the side-by-side testing in homing experiments on English and German stocks of pigeons at Cambridge, England, and Wilhelms-haven, Germany (46, 47, 48). Whatever the differences are caused by they seem to be the expression of some unknown peculiarity of the locality and not of the pigeons used. Experiments on the homing of bats have been conducted over many years and have begun to raise problems akin to those in the ornithological field. All the new information about the acoustic orientation of bats does not seem to suggest any plausible explanation. The matter is discussed by Griffin (25).

The migratory and homing orientation of fishes poses, as has long been realized, the most acute problems for the student of learning. Fish recognize their home grounds by chemical, tactile, visual, and, probably, acoustic characteristics (24, 45). Homing, as distinct from migratory movements, seems to be much more common among fishes than is generally appreciated. White bass transported to the middle of Lake Mendota, Wisconsin, swam directly towards their spawning site on the north side of the lake, apparently navigating by a sun compass mechanism (33) [see also (34)].

The orientation of invertebrates, although at first sight perhaps better understood than that of fishes, still raises challenging problems. Some observations on the maintenance of direction by migrating Lepidoptera (111) seem at present to defy physical explanation. The famous problem of honeybee orientation has been a step further clarified by the discovery of specialized proprioceptive organs mediating the accurate perception of the vertical upon which the orientation dances depend (65). Further study of the sun orientation in bees has, however, revealed some discrepancies. Kalmus had concluded that bees are innately able to calculate the sun's course in relation to their own orientation either clockwise or counterclockwise, but Lindauer (64) finds evidence that every bee must learn the apparent course of the sun for the appropriate season and locality. It has been abundantly established now that a wide variety of terrestrial and aquatic arthropods can utilize the polarized light of the blue sky for determining geographical direction, but a surprising doubt has recently arisen as to the mechanism by which the compound eye is able to perceive the plane of polarization, and it has even been questioned whether the observed responses could not be entirely explained by apparent brightness difference caused by differential scattering or reflection (107). Full understanding of the achievements involved in the methods of orientation used by the honeybee will not be achieved until this matter has been finally cleared up.

#### RHYTHMS OF BEHAVIOR

The problem of innate and conditioned rhythms of behavior is a field of such importance that there is not space even to list the relevant papers here. During the period, however, some important general reviews have been published (4, 28, 80).

The triggering or setting effect of the first stimulus or stimulus-alteration by which these rhythms are often set going can be regarded as a special case of the early sensitivity to particular stimuli which is well known in many of the higher animals, particularly birds, under the term of imprinting. Thus imprinting, priming, triggering, and one-trial learning all have elements in common, and a comparative study of the mechanisms at work in all these cases may prove to be a very profitable line of investigation for the future. This matter, with special reference to imprinting, has been reviewed by Thorpe (96). A complementary review of the subject with special reference to the parent-offspring relation in birds and mammals is given by Hinde (43), and particularly important discussions of the evidence for the critical period hypothesis and a review of the latest work with special reference to the degree of permanence of the imprinting effect are contributed by Hess & Schaefer (36) and Hess (35). These current reviews render it unnecessary to list the many important contributions that have appeared.

Exception should perhaps be made by referring to the work of James

(51) which provides evidence for the hypothesis that retinal flicker acts as an unconditioned stimulus for imprinting in barred Plymouth Rock chicks. These chicks will approach an intermittent light source with an alacrity which increases with practice, and if a stationary conditioned stimulus is placed near the intermittent light source, the chicks will subsequently follow the conditioned stimulus up and down the runway. The implications of imprinting for socialization constitute a field which has as yet been little studied, but it has been shown (27) that socially reared chicks follow models poorly but tend to become imprinted on each other, this socialization effect apparently determining the end of the imprinting period. The importance of socialization processes allied to imprinting leading to a unification of group behavior, "the establishment of traditions," has been studied by Klopfer (54, 55, 56) and others under the title of empathic learning or observational learning in birds. Klopfer showed that in greenfinches the feeding response can be established more readily than an avoidance response, apparently as a result of conditioning, the unconditioned stimulus being the sight of another bird feeding. The suggestion is made that birds showing this type of learning pattern will prove to be conservative in their feeding habits when compared with more opportunistic species whose learned avoidance responses should be more stable. This characteristic species behavior may be determined by imitative processes and the establishment of traditions as well as by innate differences in structure and behavior. Miyadi (75) has shown how some isolated colonies of Japanese monkeys may develop strikingly different food preferences. Once established, these preferences can be made stable through ensuing generations. This phenomenon is of importance for the student of selection and, theoretically at least, is capable of affecting the course of evolution in certain groups. Very relevant to this work on birds are the results of Church (17) who found a conditioned response interpretation for some cases of "sympathy."

#### MISCELLANEOUS

There are other aspects of the subject in which important work has proceeded during the period under review but which can also be passed over lightly in view of the publication of recent comprehensive critical discussions. One of these is the general topic of exploratory behavior which has recently been evaluated critically with a very complete list of references by Barnett (7). One study of particular interest published since this review supports the view that in cats manipulatory and exploratory activities are rewarding in their own right and that the postulation of a derived incentive function is unnecessary (74). Barnett in his study points out that exploratory learning in rats gives the maximum of information about the environment in the safest possible way and thus has important survival value. He also points out that the study of exploratory behavior has thrown new light on motivation since it has shown that exteroceptive stimulation has a neces-

sary arousal function that influences the animal's general level of activity.

A cognate problem, that of the place of reinforcement in explanation of behavior, has been surveyed by Watson (108). He points out that in practice we often cannot answer the question as to what are reinforcement conditions without making initial assumptions about the drive states. The drive account of a motivational mechanism suggests that what occurs during learning is the connection of stimuli with responses. If, on the contrary, we postulate that the goal is in fact certain special stimuli or, as the ethologist would put it, the animal is seeking the perception of consummatory stimuli rather than the release of a consummatory act, we then tend to regard learning as more in the nature of association between cues. Coming by way of evidence that either not all learning is dependent upon reinforcement or that reinforcement itself does not always consist in drive reduction, he provides new and convincing arguments for the view that exploration is not solely explicable as an expression of the primary drives. Watson indicates a valuable new viewpoint by suggesting that when an animal is satiated for its primary incentives, that one is selected for the goal of the behavior which is least associated with other cues. The implication of this is that the consummatory response theory, which was implicit in the work of Lorenz and is more precisely expressed in that of Sheffield and Tinbergen, cannot be a generally applicable explanation and that no simple drive reduction hypothesis will meet the facts as now known.

The teasing problem of the number sense in animals has been carried further. Lögler (66) provides some remarkable evidence pointing to true counting in the parrot. Wesley (110) trained rats successfully to "twoness" and "threeness" but the response to the latter was not maintained after the exclusion of "triangularity." Rossmann (87) was unable to prove the ability to "count" in fish.

Some of the problems of developmental changes in learning capacity have been reviewed by Vince (101) who points out how difficult it is to be certain whether learning ability really changes in a specific way with age or whether the changes we observe may not, in fact, be due to other variables. This brings us back to some of the evidence for critical periods in development. While the evidence for many of these periods is clear enough, that for others is of doubtful reliability and Vince emphasizes the need for more profound analysis of learned behavior into its component elements before the real conclusion can be arrived at. She points out that behavior in young animals can obviously be affected, first, by the general level of activity on which the degree of responsiveness may directly depend and, second, by the ability to control actions by precisely timed inhibition. There is no doubt that the full significance of inhibitory processes in the developmental aspect of learning has been unduly neglected. Tasks requiring ability of the kind which is determined by a high degree of activity and therefore of responsiveness are likely to be easier for younger juveniles, while those re-

quiring the second type of achievement—namely the type of control manifested by internal inhibition—are likely to be easier for the older ones. Vince's own work is concerned primarily with birds, but her conclusions are applicable to a wide range of other organisms.

Harlow & Woolsey (31), aided by 18 collaborators, have provided an important contribution towards a fuller understanding of the biochemical and neurophysiological bases of behavior. The chapters by Beach, Sperry, Brady, and Hebb are particularly relevant to the subject of the present review. Finally, Verplanck's "Glossary" (100) will be helpful to many students of ethology and comparative psychology.

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## SCALING AND TEST THEORY<sup>1,2</sup>

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The chapter heading, "Scaling and Test Theory," is new to the *Annual Review*, but the topic itself is not. In previous volumes, authors of the chapters on statistics have routinely devoted up to half of their space to material in this field. Emphasis in the past, of course, has been on those aspects of psychological measurement most closely related to statistics. As a result, much of the more experimentally oriented work on scaling has been covered only casually. In the present review the emphasis is reversed.

Several books of more than usual interest have been published during the past year. Churchman & Ratoosh (17) edited a volume of papers originally presented at a five-session symposium on measurement held during the 1956 meetings of the American Association for the Advancement of Science. The volume is divided into four parts: meanings of measurement, theories of measurement, problems in the physical sciences, and problems in the social sciences. Except, perhaps, for the physical sciences section, each part contains papers directly relevant to psychological measurement. Gulliksen & Messick's *Psychological Scaling: Theory and Applications* (51) is a series of papers presented at another conference on measurement. This one, held at Princeton in 1958, covers new developments and ideas in nearly all branches of psychological scaling. Chapters of both volumes are discussed separately in appropriate sections of this review.

Thurstone's *The Measurement of Values* (129) is another important volume of papers on psychological measurement. The volume brings together in a single source Thurstone's many original contributions to the theory and methodology of scaling. Most of the chapters are concerned with the development, extension, and applications of a single general model to different experimental procedures and to different content areas. Thurstone's judgment model gave us a single rationale for relating the methods of paired comparisons, rank order, and category rating and sorting. It has been used for measurement of psychophysical attributes, attitudes, values, and preferences. Essentially the same general notions have also served as a basis for multidimensional scaling models, detection theory, and one of the general models for measurement of ability (130).

Luce's *Individual Choice Behavior* (82) presents us with an alternative general model. In this volume, Luce develops his simple, but powerful,

<sup>1</sup>The survey of the literature pertaining to this review was concluded in May, 1960.

<sup>2</sup>Abbreviations used in this chapter include jnd (just noticeable difference).

<sup>3</sup>Operated with support from the U. S. Army, Navy, and Air Force.

choice axiom and explores its consequences in psychophysical scaling, detection theory, utility theory, and learning. Again, a major value of the approach is that it gives us a single rationale for examining many heretofore diverse problems connected with choice behavior. The particular scaling procedure that results as a deduction from the basic axiom has been used before. Extensive literature, beginning with Bradley & Terry (14), is available for the paired comparisons case. The logarithmic version of the scale is essentially equivalent to that obtained using Thurstone's methods, differing only by substitution of the logistic curve for the normal ogive.

Beecher's (8) outstanding treatise on the measurement of pain will be of great value to psychologists. The volume not only presents his own approach to the subject, but also contains an excellent, comprehensive review of the literature. Attneave (6) presents a brief outline of the concepts and techniques of information theory, along with an extensive review of its applications to psychological problems.

#### LOGIC OF MEASUREMENT

Stevens (118) traces the development of his views on measurement and adds the "logarithmic interval scale" to his set of scale types. The logarithmic interval scale is based on three operations: determination of equality, of order, and of equal ratios. The new scale thus differs from an interval scale by the substitution of an equal-ratio operation for the equal-interval operation. It seems to the present reviewer that the two scale types are completely equivalent. In the absence of outside criteria, any operation that can be interpreted as resulting in equal ratios can, as well, be interpreted as resulting in equal intervals instead. Any experimental tests that one can make for the equal-ratio hypothesis can be translated directly into equivalent tests for an equal-interval hypothesis. Since the two scale types are thus completely isomorphic, choice of one over the other cannot be made on empirical grounds.

Baggaley (7) suggests adding to Stevens' classification a distinction between dichotomous nominal "scales" with only two categories and those with more than two categories. Certain statistics are often applicable to the former but not to the latter. Other general discussions of the nature and logic of measurement are given by Caws (15), Menger (95), and Suppes (122).

The rather astonishing consequences of an innocent appearing "principle of theory construction" are presented by Luce (84). The paper should be read carefully, not only because the material deserves careful study, but also because it seems prone to misinterpretation. The principle involves the notion that an admissible transformation made on the independent variable should require only an admissible transformation of the dependent variable to keep intact the form of the substantive law relating the two variables. For example, given that both variables are ratio scales, if the independent variable is transformed by multiplying all values by a positive constant, then

multiplication of all values of the dependent variable by an appropriate positive constant (the only admissible transformation of a ratio scale) should keep the form of the law relating the two variables the same. The forms of substantive equations relating the two variables which fulfill the requirements of the principle are given for each of the nine pairs of scale types considered (all combinations of interval, ratio, and logarithmic interval scales). For example, if both variables are ratio scales, the equation relating them must be a power function. If the independent variable is ratio and the dependent variable interval, then either a power function or a logarithmic function will serve. If both are interval scales, the relation must be linear. The last half of the paper, which is fully as important as the first, discusses ways in which physicists and others have by-passed the conclusions presented earlier. One of the procedures is that used in the usual statement of Fechner's law,  $y = a \log (x/x_0)$ , where the independent variable enters into the equation as a dimensionless ratio.

#### PSYCHOPHYSICAL SCALING

Over the past few years, interest has revived in the traditional question of the form of the psychophysical law relating a subjective attribute to its physical correlate. The reopening of the question is largely due to the work of S. S. Stevens, whose most recent discussions of the problem are given in Stevens (117, 119, 120). Psychophysical scaling methods are classified into three types: discriminability methods, category methods, and ratio methods. Discriminability scales include those developed from summation of just noticeable differences (jnd), the paired comparisons scales, and successive intervals scales. These methods require no more than ordinal judgments from the subject. The category class includes the methods of equisection, equal appearing intervals, category rating and category production. The ratio class includes the methods of ratio estimation, ratio production, magnitude estimation and magnitude production. For prothetic attributes, the three classes yield different functions for the psychophysical law. Discriminability methods result in a function that is roughly logarithmic; ratio methods result in a power function; and category methods yield a function that usually falls somewhere in between. Which, if any, represents the proper form of the law? Stevens, though willing to admit that the choice is a matter of opinion (117), argues persuasively in favor of the power law and the associated ratio and magnitude methods.

During the past year, papers by proponents of the ratio methods have been devoted mostly to development of ratio scales for new psychophysical attributes and to cross-modality validation of the scales developed so far. Thus Stevens & Stone (121) present ratio, category, and jnd scales for "finger span"—subjective thickness of blocks of wood held between the thumb and third finger. The magnitude methods gave a power law with an exponent of 1.33. When plotted against the ratio scale, the category and jnd scales showed the expected curvature, i.e., the jnd scale was more curved

than the category scale. Stevens (116) presented similar data for intensity of tactile vibration on the finger and on the arm. The power law for the finger had an exponent of .95. Again, category and jnd scales showed the expected curvature. When the magnitude procedure was used on the arm, the resulting function departed somewhat from the simple power relation. Addition of a constant to adjust for origin straightened things out, however. For this attribute the more general form of the power law,  $S = C (X + X_0)^k$  is thus necessary. Other studies are those of J. C. Stevens & Mack (113) on scales of apparent force, Geisler *et al.* (44) on loudness of clicks, Chatterjea & Ramanath (16) on lifted weights, Ekman, Eisler & Künnapas (32) on brightness of different monochromatic lights, and Björkman & Holmqvist (13) on subjective time. S. S. Stevens (120) displays a representative sample of over 20 attributes that result in power laws when scaled by the ratio or magnitude methods. The exponents of the function vary from .33 for brightness of a 5° target to 3.5 for electric shock.

Galanter & Messick (38) developed magnitude, category, and successive intervals scales for loudness of noise. A major purpose, following a suggestion by Messick & Abelson (94), was to determine if treatment of category data by the general successive intervals model would result in a linear relation between the category and magnitude scales. The result was just the reverse: the successive intervals scale turned out to be logarithmically related to the magnitude scale. They recommended that an antilogarithmic transformation be applied to the successive intervals scale, on the assumption that the subject's judgments actually reflect sensation ratios rather than sensation differences.

The cross-modality validation studies are based on the following notion: given that the power functions for two different attributes are of the form  $S_1 = X_1^k$  and  $S_2 = X_2^c$ , if the subject is presented with a series of stimuli from the first attribute ( $X_1$ ) and, for each stimulus, told to adjust the intensity of the other attribute until it matches the intensity of the first, then the resulting pairs of stimulus values should be related by an equation of the form

$$X_1^k = X_2^c, \text{ or } \log X_1 = \frac{c}{k} \log X_2.$$

The  $c$  and  $k$  are the exponents obtained using one or more of the ratio scaling procedures. The empirical check is to determine whether direct cross-modality matching will result in an "equal-sensation" function which, when plotted in logarithmic co-ordinates, has the required slope of  $c/k$ . Stevens, Mack & Stevens (114) show results when seven different continua are matched with force of handgrip. In all cases the obtained slopes closely approximate the theoretical values. S. S. Stevens (115) applied the procedure to all combinations of loudness, vibration, and electric shock and, in another experiment (116), compared tactile vibration on the arm with that



on the finger. Similar results were obtained. In the latter two studies, departures of one of the variables from the strict power law resulted in cross-modality matches that departed accordingly.

What more could we ask? There is first of all the matter of variability. Though the magnitude scales have been based on medians or geometric means of judgments from groups of subjects, the differences between subjects are rather impressive. Garner (41) suggested that, whereas the subject can behave consistently in adjusting stimuli to a prescribed ratio, the actual numerical value of the ratio should be treated as an unknown. He used a combination of equisection and fractionation methods to obtain a loudness scale in which individual differences were markedly reduced. In a more recent paper (43) he showed how his loudness scale relates to the phenomenon of masking and to the loudness of multicomponent tones. McGill (92), using a graphic rating method, attacked the problem somewhat differently. He found it necessary to adjust each subject's "zero point" before the individual curves became linear on a log-log plot. Even so, the slopes differed considerably, a fact which he related to the "rubber scale" phenomenon described by Rogers (104) and McGarvey (91).

The ratio scales also have the rather unfortunate property that differences that are supposed to be subjectively equal, according to the scale, simply do not (subjectively) appear to be equal. As Stevens says, "A given difference that is large and obvious near the low end of the scale is much less impressive in the upper part of the scale" (120, p. 53). Indeed, it is not unusual to find that stimuli spaced in equal subjective intervals according to direct judgments of differences are judged to be separated by very nearly equal ratio steps when one of the magnitude methods is used. Garner (41) found that over half of his subjects made no distinction between the tasks of equating intervals and equating ratios. Torgerson (131), in a series of experiments using the category and magnitude methods on both lightness and darkness of Munsell color chips, obtained much the same result: equal intervals on the category scale corresponded closely to equal ratios on the magnitude scale. Further, when lightness scales were compared with darkness scales, the category scale of lightness was found to be linearly related to that for darkness, whereas a reciprocal relation was obtained for the corresponding magnitude scales. In the category scales, differences were invariant over reversal in direction of judgment; in the magnitude scales, ratios were invariant. The results suggest that the subject perceives a single quantitative relation between stimuli, which he interprets as either an interval or a ratio, depending upon what the experimenter tells him to do. If this is true for other prothetic attributes as well, the question of the form of the psychophysical law promises to be with us a long time to come. Perhaps Kryter's (69) compromise is the best way out. He constructed a scale of loudness of airplane noise that fits the power law notions and then immediately defined a "noisiness-level" (decibel) measure for it that fits the logarithmic law.

## ABSOLUTE JUDGMENT: CONTEXT, ADAPTATION, AND INFORMATION

Absolute judgement is often used more or less as a synonym for category rating. Context effects in absolute judgment experiments refer to those factors that demonstrate that such judgments are by no means absolute. Helson (58) gives an up-to-date presentation of adaptation-level theory, an attempt to account quantitatively for shifts in the location parameter (adaptation level) of the subjective scales under various anchoring conditions. Helson & Nash (59), in an experiment on the effects of background stimuli on absolute judgments of several sets of stimuli, found that, within each set, stimuli nearer the background were affected most, whereas when each set is considered as a unit, the sets farthest from the background stimuli were most affected. Results were explainable in terms of the theory. Thurlow & Tabory (127), however, and also Parducci (100), obtained results which they feel are not accounted for by adaptation level. Thurlow & Tabory obtained judgments of tones presented 100 times in succession. According to the adaptation-level notion, ratings of loud tones ought to decrease with time and those of soft tones ought to increase. The changes, if any, were in the reverse direction. Parducci's study of ordinal effects in judgment indicated that the relation between number of categories and number of stimuli was an important variable.

Garner (42), in a follow-up of an earlier study on context effects in half-loudness judgments (40), suggested that the strong context effect operating in this situation is primarily attributable to a general response set rather than to the particular set of stimuli used.

Since Hake & Garner's paper (53), a considerable number of studies have been carried out to determine the amount of information transmitted by absolute judgments for various psychophysical attributes. Much of the material is reviewed in Attneave (6) and Miller (96). During the past year, two new modalities were investigated. Engen & Pfaffmann report results on judgments of odor intensity (35) and quality (36). Transmitted information was about two bits for intensity alone and somewhat less than four bits for quality. The authors comment on the surprisingly low value for quality and suggest that the popular notion of the human's great capacity for discriminating and remembering odor qualities is probably wrong. An alternative explanation might have to do with the sorts of stimuli available to laboratory experiments on odor. It may be that the qualities we discriminate and remember well are not those that are easily put into bottles. Information transmission for absolute judgments of intensity of electrical stimulation applied to the skin was studied by Hawkes (54) and Hawkes & Warm (55). Information transmitted was less than two bits. An incidental point worth mentioning is their attempt to control optimally the effects of stimulus spacing [in line with suggestions by Garner (39) and Alluisi (4)] by spacing the stimuli in equal subjective steps. However, the subjective scale they used was one derived through use of the magnitude estimation procedure. Equal spacing on such scales does not

correspond to equal spacing on the equidiscriminability scale, as recommended by Garner, or even to equal spacing according to direct judgments of subjective intervals.

#### DISCRIMINABILITY

*Successive intervals.*—It has become customary to assume that psychological ratio scales can be obtained only through use of the ratio-type of judgment, that scales based on equisection judgments or on variability are necessarily no more than interval scales. Such is not always the case. A number of different procedures have been devised that convert interval scales to ratio scales. One of the more recent and interesting approaches is that of Cliff (18) on the influence of adverb modifiers on the values of adjectives scaled on a "favorableness" dimension. Cliff's model adds to the usual successive intervals procedure the notion that common adverbs of degree multiply the favorableness or unfavorableness of an associated adjective. Thus, for example, if "admirable" and "contemptible" have ratio scale values of  $s_1$  and  $s_2$  when unmodified, the scale values of "very admirable" and "very contemptible" would be given by  $as_1$  and  $as_2$  where  $a$  is a positive number associated with the adverb "very." The model was tested using 15 adjectives rated alone and in combination with each of nine adverbs on three groups of subjects. The data enable one to solve for the multiplying value of each adverb and the scale values of each adjective and adverb-adjective combination on a ratio scale. The fit of data to theory was very good. A second experiment, using the same notions added to the method of paired comparisons and the law of comparative judgment, also produced a close fit. Thus, on Cliff's scale, both ratio relations and interval relations are empirically significant.

Dudek (28) repeated a portion of Cliff's study using the constant sum method—a ratio-judgment procedure. He found that the constant sum scale values for adverbs were nonlinearly related to Cliff's values for the same adverbs. The curvilinear relation obtained was similar to that found between category and ratio methods on psychophysical attributes. Unfortunately, no observations across adjectives were obtained. Hence, a direct test of the over-all consistency of the multiplicative rule for this procedure is not yet available.

Jones (65) presented an impressive list of conditions under which successive intervals scales have been demonstrated empirically to remain invariant. He also presented data showing a linear relation between the successive intervals scale and a new scaling procedure devised by Thurstone (129, Chap. 16) that avoids the usual normality assumption of successive intervals when the model is applied across subjects.

The usual version of successive intervals involves solving for the category boundaries. A modification of the model that uses category means rather than boundaries has occasionally been recommended. Green (47) demonstrated that the modification is logically incorrect.

*Paired comparisons.*—Gulliksen (50) described an IBM 650 program for analysis of a complete paired-comparisons schedule for any number of stimuli up to 21.

The experimental labor required of a subject for a complete set of paired comparisons becomes excessive when the number of stimuli becomes at all large. The alternative of requiring the subject to rank order the complete set (from which the paired comparisons can be deduced) becomes excessively difficult as the number of stimuli becomes large. Compromise procedures that reduce the number of observations required, without increasing excessively the difficulty of the task, involve use of the rank-order procedure with subsets of stimuli. The problem of selecting subsets such that all stimulus pairs appear together the same number of times has received attention during this reporting period. Schucker (106) described a procedure for obtaining sets of triads such that all possible pairs of stimuli appear together just once. The routine requires that the total number of stimuli be an odd multiple of three. Gulliksen & Tucker (52) list a number of experimental designs from Cochran & Cox (19) that also result in each stimulus being compared with each other stimulus just once. They described an IBM 650 program for processing data for the case of 31 objects grouped into 31 blocks of six stimuli each. The other designs referred to range from seven stimuli arranged in seven blocks of three each to 57 stimuli arranged in 57 blocks of eight each. The subject's task in all of these designs, of course, is to rank order the stimuli within each block.

An alternative procedure for reducing the experimental labor of paired comparisons is to use only a subset of pairs of stimuli. McKeon (93) discussed some cyclical incomplete designs for paired comparisons, and Rambo (101, 102) presented empirical data on effects of such partial pairing.

Gridgeman developed a paired-comparison model for hypothesis testing that allows for ties (49) and a probabilistic model for sorting stimuli into two classes (48). Coombs (22) presented material relating the notions of his unfolding model to inconsistency of preferences. The relation between inconsistency and psychological distance was found to depend upon whether both stimuli are on the same or opposite sides of the subject's ideal. Luce (85) developed a unidimensional choice model for the complete method of triads, based on a similarity version of his choice axiom. He pointed out that his triads model is consistent with ideas presented by Coombs.

*Signal detection theory.*—Applications of signal detection theory to psychophysical problems will not be covered in detail; however, two papers of general interest should be mentioned. Swets (124) showed that  $d'$ , the detection theory measure of sensitivity, is invariant over a number of different psychophysical procedures, whereas the usual threshold measures are not. Egan, Schulman & Greenberg (31) compared results of experiments using two-category judgments under various strictness criteria with one using four-category judgments. Boundaries between successive categories

in the four-category condition corresponded to the various strictness criteria of the two-category conditions. Equivalent results were obtained for the two procedures.

#### MULTIDIMENSIONAL SCALING

The notion of "psychological distance" is basic to multidimensional scaling. A good general discussion of distance, multidimensional scaling, and types of behavioral data amenable to the procedures is given by Shepard (108). Luce (85) in developing his model for the unidimensional method of triads, found that only the logarithmic version of the scale implied by his choice axiom was suitable for measurement of psychological distance—the ratio version of the scale was not. Torgerson's results (131) suggest that differences on a magnitude estimation scale of an attribute are also not suitable measures of psychological distance, since with these methods the relative size of differences depends upon direction of judgment. This does not say, however, that direct magnitude or ratio estimations of the distances themselves are not appropriate. It has been argued for quite some time that the usual successive intervals and triads procedures underestimate the larger distances. Helm (56) discovered that an exponential transform of successive intervals scales of distances between colors gave results more nearly in line with those of the Munsell system. The unidimensional results relating successive intervals to magnitude estimation suggest that the magnitude method used on distances might serve to overcome the bias as well. Helm (57) used a multidimensional ratio procedure described in Torgerson (130) and found a good fit for color data without using any transform. Indow and co-workers used a graphic rating procedure for obtaining distances between colors varying in hue and chroma (62) and in hue, chroma, and value (61). Again, the multidimensional results were in general agreement with the Munsell system, although the spacing between stimuli differed somewhat.

Applications of the standard procedures to new areas were made by Morton (97) and Reed (103). Morton used multidimensional scaling to determine the friendship configuration for two different fraternity groups. Dimensions obtained were related to traits characterizing the subjects. Reed, with 25 Youth Employment officers as subjects, used the approach to obtain a multidimensional configuration of judged similarity of 15 jobs. Eighty-seven per cent of the variance of the original distances could be accounted for by only two factors—a crafts vs. clerical dimension and a general level dimension.

Bennett & Hays (9) and Tucker (135) presented alternative multidimensional models for analyzing preference data. Bennett & Hays' distance model assumes that the subject's preference orders reflect distances of the stimuli from the subject's ideal. The model is, thus, a generalization of Coombs' unfolding model. Tucker's is a vector model, where observations

of individuals' preferences are related to the scalar products between subject vectors and stimulus vectors. Bennett & Hays' model is nonmetric, requiring only rank orders of preference. Tucker's is a metric model and, at least in his applications, assumes that the subjects form a normal multivariate distribution in the psychological space. Tucker shows that the projections of the stimuli on the average subject vector of his model correspond to the usual law of comparative judgment scale values. Coombs (23) shows an idealized relation between the unfolding procedure and the usual factor analysis of preference ratings. Factor analysis results in one extra dimension, which corresponds to over-all preferability of stimuli.

#### SEMANTIC DIFFERENTIAL

Jenkins, Russell & Suci (64) have prepared a table of distances between all pairs of the 360 words included in their earlier atlas of semantic profiles (63). It should be noted that the computed distances are distances between concepts in a 20-dimensional space defined by considering each scale as an orthogonal axis; they are not distances in the common factor space defined by the 20 scales. Norman (99) studied several stability characteristics of the semantic differential, holding concepts and scales constant and examining variability over occasions and over comparable subject groups. Consistency of individual ratings was not so good, factor scales for individual raters were not much better, and consistency of individual semantic spaces was poor. Consistency across groups of subjects, however, was high. Group mean *D*-values and group mean ratings were both highly reliable.

Abelson (1) described a discriminant analysis approach to problems typified by the semantic differential that enables the experimenter to separate out the different components of variance.

#### GAMBLING, UTILITY, AND SUBJECTIVE PROBABILITY

Edwards (30) reviewed a number of models devised for measuring utility and subjective probability in gambling situations. His concern is with those models that require utility to be measurable on at least an interval scale and assume that the subject behaves as though he were maximizing some kind of expected utility or value. For models that assume the subject maximizes subjective expected utility, he shows that either the subjective probability must be linearly related to objective probability, or it can not be additive. His own experiments, along with others, indicate that the latter is the better choice. Experiments during the past year by Nogee & Lieberman (98) and by Komorita (68) add weight to the conclusion.

Luce (82) applied his choice axiom along with his decomposition axiom (83) to the gambling situation. His model allows for both perfect and imperfect discrimination, as does a model presented by Suppes & Walsh (123). In Davidson & Marshak's stochastic model (26), however, perfect discriminations are not really allowed.



The gambling experiment is a difficult one to interpret, since subjective probability of events and utility of payoffs are not the only reasonable variables that can operate to determine a subject's behavior. Royden, Suppes & Walsh (105) devised a model for measurement of the utility of gambling itself. Scodel, Ratoosh & Minas (107) studied personality differences of people who preferred low-probability gambles with a high payoff as against those who preferred the reverse. They conclude that bettors who prefer high-probability, low-payoff bets are more other-directed, more socially assimilated, and more middle-class oriented. Other variables are considered by Siegel (109) and Siegel & Goldstein (111) in their study of the Humphrey's two-choice uncertain outcome problem. Their expected-utility models for this situation include such variables as utility of varying one's responses, utility of reproducing stochastically the information in the event system, and differential utilities for predicting correctly less frequent and more frequent events. Their models—and the experimental results—indicate that about the only time the subject's response probabilities will match the event probabilities is when the payoff associated with correct guesses is small enough so that the other types of utilities become important.

One of the predictions of Luce's model is that under certain circumstances the probability of choosing a particular option increases from zero to one in a series of discrete steps with an increase in the subjective probability of the occurrence of that event. The prediction was tested by Luce & Shipley (86), with results tentatively interpreted as supporting the model.

Siegel (110) discussed his gambling model for obtaining a higher-ordered metric scale of utility. In Siegel & Shepard (112), the procedure was used to construct an ordered metric scale of social distance. Fagot (37) presented a model for determining a higher-ordered metric scale directly. Instead of using gambles to determine whether a subject prefers  $a$  to  $b$ , or whether the difference in utility between  $a$  and  $b$  is greater than that between  $c$  and  $d$ , one just asks the subject. A similar riskless choice model for stimuli that vary on more than one dimension was developed by Adams & Fagot (2). Stevens (118) recommended using direct magnitude estimations to obtain scales of utility.

#### TEST THEORY

It seems to this reviewer that not too long ago the heart of test theory, or rather, true score theory, was a neat and straightforward thing. One started with the definition of true score,  $t = x - e$ , assumed errors were normal and independent, and in a very few pages derived or defined most of the basic notions. Those days, if they ever existed, are certainly gone now. Test theory today can boast of several kinds of true score models, many reliabilities, and a host of wonderfully complex mathematical problems.

Lord, in his presidential address to the Psychometric Society (75),



described five different true score models: the usual Gaussian error model; a similar binomial error model; a rationally-equivalent-forms model for the Kuder-Richardson approach; a matched-forms model that assumes only that the expected value of error is always zero, but requires  $k$  tests, parallel in the sense that true score of each subject is the same on each test; and an item sampling model, which assumes the items of a test are a random sample from a large pool of items. In other, more technical papers (73, 77, 79, 81) Lord developed formulas for different models for estimating true scores of individual subjects and for estimating the moments and cumulants of the distributions of true scores and errors of measurement. Lord (80) developed procedures and then showed empirically that the usual Gaussian model was inappropriate for a set of four widely differing groups of subjects. Distributions of errors of measurement were not normal, nor were the errors of measurement uncorrelated with true score.

Use of true score theories to justify or provide a meaningful rationale for reliability coefficients and the like is probably desirable and proper. The value of the true scores themselves as measures of psychological attributes is not so clear. They seem to represent a sort of a halfway house between testing problems of a strictly practical nature and those of more scientific interest. True scores themselves are not ordinarily needed for problems of the first kind and are not really adequate for many of those of the second kind. One special condition should be noted where they do seem adequate: where the items of the tests are really samples drawn from a genuine population or universe of items, as in the spelling-test example described by Lord (78). Here, the true score is simply an estimate of the proportion of items in the population (e.g., a given dictionary) that the subject can answer correctly.

Under most circumstances, however, true scores are ordinal measurements. Relative differences between true scores are not invariant either over tests of a given ability [Lord (75, 81)] or over samples in which the subjects find themselves. For purposes of psychological theory, measurement of attributes on interval scales, such as those obtained from Lord & Tucker's normal ogive model, Lazarsfeld's latent structure models, or Birnbaum's logistic model, is to be preferred. Of course, the models required for interval scales do involve more restrictive assumptions. However, since the models are accompanied by criteria for evaluating goodness of fit, such assumptions are to be sought rather than avoided.

Lazarsfeld's (70) general discussion of latent structure analysis and its relation to Lord & Tucker's ability model was the only paper found dealing with the continuous version of the latent structure model. No papers were found on the normal ogive traceline model. Maxwell (89), however, gave maximum likelihood estimates for the location and discriminability parameters of items with logistic tracelines. The procedure, which seems to as-

sume that the distribution of subjects is known, is related to the more general logistic models developed by Birnbaum (10, 11, 12).

*Reliability.*—Edwards (29) showed that Tryon's general formula for reliability (132) is algebraically the same as two analysis of variance versions given earlier by Hoyt (60). Lord (74) took issue with some statements made earlier by Lyerly (87) concerning the "basic" assumptions of the Kuder-Richardson formula 21 for reliability. We have here an illustration of the danger nowadays of beginning any sentence in test theory with the phrase, "The basic assumption for. . . ." Most of the indices of test theory can be arrived at by markedly different routes. Lyerly (88) presented significance levels for the K-R 21 formula, and Englehart (34) compared procedures for reliability of ratings with those for tests using an analysis of variance approach.

Lord (76) showed that, for 58 tests, the usual standard error of measurement for a test was approximately given by  $3\sqrt{n}/7$ , where  $n$  is the number of items in the test. Swineford (125) developed empirical formulas for estimating test standard deviation and reliability from item statistics and, in a note (126) on Lord's paper, showed that for tests having the usual sort of item statistics,  $3\sqrt{n}/7$  is indeed about what one would expect. Davis (27) presented standard error of measurement formulas for eight different kinds of score differences.

Cronbach & Gleser (24), in a note on an earlier paper by Lord (72) concerned with the usefulness of difference scores of low reliability, suggest a decision rule for the counseling situation based on maximum risk of misinterpretation rather than the average risk considered by Lord. The alternative rule would lead to a more conservative estimate of the usefulness of unreliable scores.

*Item analysis.*—Adams (3) compared the reliability of several commonly used item validity indices across equated groups of subjects. The tetrachoric coefficients and the upper-lower 10 per cent coefficient fared the worst. Colver (20) presented a nomograph for the Flanagan & Davis item indices.

*Regression analysis.*—Anderson & Fruchter (5) compared the Doolittle, Wherry-Doolittle, and Summerfield-Lubin procedures for computing multiple correlations and for selecting tests. All three are equivalent with respect to the computation of the coefficient, and the latter two were also found to be equivalent with respect to selection of variables. They recommend the Summerfield-Lubin procedure because of the meaningfulness of the interim computational values. Linhart (71) developed a statistical criterion for deciding whether to use an additional predictor variable (or any predictor variable) in a multiple regression problem. Elfving, Sitgreaves & Solomon (33) gave a procedure for selecting items when factor loadings of the items and the criterion are known.

## FACTOR ANALYSIS

Tryon (133) formulated a "general method of multidimensional analysis" from his domain sampling principles. The general method is essentially a list of those steps involved in ordinary factor analysis that require decisions from the analyst. Tryon's table shows how differences between various cluster and factor analysis procedures depend upon the particular decision made at each step. All major factor analysis methods are treated as special variants of cluster analysis.

Wolins (137) suggested an improved formula for use in securing the factor loadings of items in the Wherry-Winer method. Wherry (136) came out with a new analytical procedure for the Wherry-Winer hierarchical factor model. The new procedure uses the multiple group method on original clusters of variables, on clusters of intercluster correlations, on inter-correlations of these clusters, etc., until the final correlation matrix consists of a single cluster. The solution gives the loadings of the original variables on the factors of each level. Simple structure is held to be achieved for the lower-level factors directly. The method is an improvement over the original procedure because no rotations of axes are required.

Kaiser (67) suggested that Tryon's solution for communalities (which was independently worked out by Kaiser) converges if, and only if, the correlation matrix has unique minimum rank communalities, a characteristic not possessed by empirical correlation matrices. Wrigley (138) attacked the notion of minimum rank as a criterion for determining communalities. He states that no satisfactory procedure yet exists and that, even if one did, it would not really be what is needed, since the minimum rank theory neglects statistical considerations.

Cureton (25) described a procedure for rotating an orthogonal factor matrix so that one axis passes through a particular point. Comrey (21) compared Kaiser's varimax method (66) with Thurstone's analytic procedure (128) for rotation to simple structure. He considered that the varimax method gave the more satisfactory result.

Gibson (46) pointed out an unresolved problem in Tucker's interbattery method of factor analysis (134). In another paper, Gibson (45) compared multiple factor analysis with Lazarsfeld's latent class model and devised a new method, called latent profile analysis, for analyzing correlations among quantitative variables. The new model is a direct generalization of the latent class model, with product moments substituted for joint proportions, and "uncorrelatedness" within classes substituted for independence of item responses within classes. Hence, all of the solutions developed for the latent class model are applicable to the latent profile model. Maxwell (90) gave a summary of available statistical tests for factor analysis, along with a good review of the literature on the subject.

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## AESTHETICS<sup>1,2</sup>

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Aesthetics has no clearly defined boundaries or directions. Any review of the subject will therefore almost certainly be selective, incomplete, and, if not wholly one-sided, at least defective in one region or another. Theories or models of learning, however much they may differ in detail, are held together by a common effort to understand changes and improvement in animal and human performance. Any model which failed to throw light on that problem would have a dim prospect of survival. Aesthetics has no such common bond or goal. Everything flourishes and survives, apparently because what some readers regard as good, bad, indifferent, meaningless or irrelevant, other readers think of in a quite different way. George Bernard Shaw once said that the more things a man is ashamed of, the more respectable he is. Students of aesthetics seem to be ashamed of nothing.

The lack of decent boundaries for aesthetics is in large measure owing to the fact that the subject falls in no man's land. Philosophers, psychologists, historians, critics, and artists wander at odd moments into the field—more often than not, way out into left field—but spend most of their time near the home base where they earn their salaries and professional reputations. But the number of excursions in all directions beyond the infield is becoming greater every year, perhaps as a result of the tremendous increase in interest in art all over the country in recent decades. Most students enter college today with far greater knowledge about one or more forms of art than their parents ever had, and since many of them are eager to find out whether anyone can explain the tenacious hold which art has gained, more teachers offer courses (32) and write books and articles on the subject. The literature is voluminous, but for a review of aesthetics in a book on psychology it is possible with a clear conscience to leave out whole sections. The reader who wants to explore a much larger area than the one covered in these pages will find an almost inexhaustible supply of information in Hungerland's painstaking and detailed bibliographies (56 to 62).

<sup>1</sup> The survey of the literature pertaining to this review was concluded in February, 1960.

<sup>2</sup> This review places major emphasis on work done during the last few years, but since the topic of aesthetics makes its initial appearance in the pages of this volume, it seemed best to give some background for modern trends by citing older references, some of which go back several decades. The writer has not tried very hard to conceal his predilections and prejudices. Impartiality is difficult to maintain when opinions and argument, often heatedly expressed, far outnumber cool correlations.

The serious student of aesthetics, even if his home base is psychology, must still turn to the philosophers for guidance, at least for a few more years. Ever since the days of the Greeks, almost all philosophers have written books or chapters on aesthetics, some of them very sharp and penetrating, and nearly all of them in need of sifting and sorting in order to find out what can be done with them by psychologists who would go at aesthetics *von unten* rather than *von oben*. Plato's insistence, for example, that it is the form rather than the content which makes a work of art great, since content is merely a copy of a copy and therefore three degrees removed from reality and for that reason unworthy of serious attention, is a view perhaps more alive today than it ever was, both in theory and, very much, in the practice of those artists who manage with bewildering success to produce works which reveal no apparent relation to anything at all. And so it is with many other doctrines in the history of aesthetics. They have been worked over and thought about by generations of philosophers and cannot be ignored by psychology until tested by some kind of empirical, statistical, or laboratory methods rather than the methods of the armchair.

There are many books of readings in the philosophy of art. For the psychologist, Rader's (97) is perhaps the best in variety and range of topics covered. Bosanquet's *History* (17) has long been recognized as an authoritative and standard work, but it is tough going in places. A far more lucid and much better organized book is *A History of Esthetics* by Gilbert & Kuhn (44).

Experimental aesthetics and psychophysics were born (if, in spite of Boring, one may be allowed to assign dates to the birth of ideas) only a few years apart, and both were among the numerous brain children of the same man. William James has said,

Fechner was the pattern of the ideal German scholar, as daringly original in his thought as he was homely in his life, a modest, genial, laborious slave to truth and learning. . . . His mind was indeed one of those multitudinously organized cross-roads of truth which are occupied only at rare intervals by children of men, and from which nothing is either too far or too near to be seen in due perspective. Patientest observation, humanest feeling, flourished in him on the largest scale, with no apparent detriment to one another (65 p. 148 f.).

The whole chapter on Fechner in *A Pluralistic Universe* is a hymn of praise in which James gives expression to his deep affection and admiration for the dear old German *Gelehrter*. This attitude on the part of James is often forgotten by psychologists, for they are more likely to remember the frequently quoted denunciation of psychophysics in James' *Principles*, where, after reviewing the dreadful literature but refusing to enumerate it even in a footnote, he said that in his humble opinion the proper psychological outcome of Fechner's work in psychophysics was just nothing (64; Vol. I, p. 534). It is obvious that James made a resounding blunder in his estimate of the importance of psychophysics, but it is conceivable that

if he had made a similar statement about Fechner's contributions to aesthetics, his words would be widely quoted with approval at the present time.

Fechner began writing about aesthetics not long after the publication of his *Elemente der Psychophysik* in 1860. A decade later an outline of his plans (30) served as a sort of preface to the principal work, *Vorschule der Ästhetik* (31). Fechner deserves, of course, the recognition he has always been given for his patient efforts to extend his psychophysical methods into the area of aesthetic judgments, but the methods had been far better elaborated in the *Elemente*, and the results themselves do not amount to much. They have little significance for the philosopher, practically none at all for the artist, and hardly any for the psychologist. Preferences for colors, tonal intervals, ratios of division of simple lines and shapes, etc. have little to do with the salient qualities of great works of art, any more than the attributes of sensation offer much help in solving the problems of space perception. Works of art are difficult to deal with in the laboratory, but some way must be found to use them more extensively as stimuli if experimental aesthetics is ever going to gain altitude. Good beginnings have been made, but there is still a long way to go. Fechner's aesthetic laws are very much the laws of Greek aesthetics: unity in variety, congruousness, balance, harmony, proportion, clearness, etc.—a large generalization from his results, but a position which places him in line with one of the most tenacious doctrines in all aesthetic theory.

In trying to move ahead, experimental aesthetics is faced with a number of handicaps in addition to the difficulties inherent in the subject itself. Philosophers, artists, and scholars in the humanities tend to resist invasion of their territory by men of scientific bent. Many of the latter must have had experiences similar to the one described by Thurstone.<sup>8</sup> Whatever the territory of aesthetics may be, this review, as stated at the beginning, will not attempt a total invasion, partly because of limitation of space, but chiefly because many topics are treated, and properly belong, elsewhere: philosophy, art history and criticism, tests of artistic talent, psychological studies (largely psychoanalytic) of the lives of artists in

<sup>8</sup> "Some time ago I attended a series of seminars on aesthetics at the home of one of my colleagues. Most of the participants in that seminar were from the humanities and the arts. The seminars were devoted to discussions about the theory of aesthetics. In some of those discussions it occurred to me that the question at issue could be treated as a question of experimental fact, and I ventured to suggest how the psychophysical methods could be adapted to obtain an empirical answer to the question at issue. It was an illuminating experience to discover that some of my friends in the humanities were hostile to the very idea of subjecting questions of aesthetic theory to empirical inquiry. On one of those occasions a friend showed me a quotation from Aristotle that settled the matter for him. It was heresy when I suggested that we knew more about this problem than Aristotle" (106, p. 193 f.).

relation to their works,<sup>4</sup> art as a therapeutic device, etc. What then is left?

More than 200 years ago the philosopher, Gottlieb Baumgarten, wrote a treatise on aesthetics (13) which, in spite of confusions in translation and interpretation, defines clearly enough and with cogent argument an area of inquiry as old as Greek aesthetics and as new as *Gestalttheorie*. The Oxford Dictionary, holding fast to aesthetics as knowledge derived from the senses, accuses Baumgarten of misapplying the word to criticism of taste and thus corrupting its proper meaning, yet gives as a second definition "the science of the conditions of sensuous perception." This definition is almost pure Baumgarten. He argued that aesthetics is perception suffused with mood and emotion. The role of emotion in art has been a major problem in aesthetics ever since it was first made troublesome by Aristotle in his concept of *Katharsis*. Baumgarten gave the role of emotion a palpable place by locating it in the field of perception. Philosophers have paid Baumgarten scant heed, and most psychologists have probably never heard of him. Yet for those among the latter who have any concern for experimental aesthetics, Baumgarten's definition furnishes the philosophical background for the promising direction much of their work is now taking.

Appreciation of art, at least the fine arts,<sup>5</sup> involves perception. Some-

<sup>4</sup> Many scholars object fiercely to the admixture of biography in the study and analysis of great works of art, science, and philosophy. They prefer to keep the genius and his work in separate compartments. Biography obviously has its proper place somewhere, but it must not be allowed to influence the evaluation of logic, style, method, theory, etc. This view has often been driven to extremes (96, pp. 57-62), but up to a point it would seem to be justified in order that works of science, philosophy, and art may be examined in their own right as self-sufficient entities. The uncritical and impressionable reader of biography may have his judgment badly warped if he learns that Beethoven was probably syphilitic, Gauguin a monster of depravity by ordinary standards, Wagner in many respects a detestable scoundrel, and that Wordsworth was the illegitimate father of the child so movingly referred to in his *Evening on Calais Beach*. At any rate, in aesthetics the work of art rather than the artist is ordinarily the object of investigation.

<sup>5</sup> The question as to what is meant by fine art has been argued almost *ad nauseam* ever since Lessing made the distinction between those arts in which beauty is revealed in the coexistence of signs (i.e., sensuous material) and those in which the signs succeed one another in time. The former—painting, sculpture and architecture—are the fine arts. The latter—poetry, drama, literature—are neither fine nor beautiful. The distinction persists today only in outmoded departmental labels still used in a few universities. Yet a related distinction exerts powerful influence in aesthetics, even when not clearly recognized. Far more attention is given to those arts in which form predominates than to those in which words are the fleeting clues to meaning and ideas—a distinction not unlike that often made in psychology between perception and cognition.

For a long time the classification of music gave trouble, but its wanderings are now about over, for although its signs succeed one another in time, the predominance of form is nearly beyond all dispute. A good many authorities would,



thing is seen or heard, but, still more important, these visual and auditory perceptions, both in art and perhaps only to a slightly lesser extent in everyday perception, seem to present to those with eyes to see and ears to hear innumerable impressions that can only be described by words looked upon as borrowed by some sort of pathetic fallacy from the vocabulary of mood and emotion. The paintings of Turner reveal agitation and violence, the music of Mozart is often wistful and melancholy in spite of the elegant gaiety of its surface, the sculpture of Michelangelo is powerful and heroic, the Renaissance madonnas are tender and sad, etc. Where do these qualities come from?

Until recent decades the answer to that question seemed simple enough in theory, even if difficult to demonstrate by fact. Moods and emotions are subjective states and therefore cannot exist in the objective—i.e., phenomenally external—areas of auditory and visual perception. To say that art embodies emotion is merely a manner of speaking. Psychologically the statement makes no sense. To call a melody sad is a judgment that involves an erroneous ascription to the tones of a quality which has its origin elsewhere. The melody cannot be sad. The sadness must be in the listener. How then does it happen that the listener so frequently makes the mistake of saying that the melody is sad? The various explanations differ in detail (94) but make common cause in their appeal to cultural influence, association, the diffuse localization of the subtler emotions as described by James (64), and above all to Lipps' doctrine of empathy. Incipient bodily movements and dispositions unite with visual and auditory impressions, giving to the latter those qualities which seem so fittingly described by words used for moods and emotions. The qualities are subjective in origin, but, by a kind of simultaneous association by inference built up through countless repetitions, they are seen and heard as aspects of objects located outside the body.

This doctrine in one form or another has probably been drawn on more frequently than any other to account for what many regard as the central problem of aesthetics, the apparent embodiment of emotion in art. The most detailed and persuasive treatment in English of empathy is in Langfeld's *Aesthetic Attitude* (71), and the arguments in that book need to be kept in mind in order to evaluate better the recent attacks directed against empathy. The doctrine is still very much alive (102) and has been carefully and sharply reformulated, although in new terminology, by Ayer (12) and especially by Stevenson in his emotive theories (103) and applied specifically to aesthetics by Garvin (36).

Serious and trenchant doubts about empathy were first expressed in some detail by the Gestalt psychologists. Wertheimer could not bring him-

in fact, assign to musical form a higher and more important place than to any other kind of form. Music has at last become a fine art, although the label is academic, or rather, nonacademic.



self to believe that the cheerfulness or solemnity of things is imported from another sense department. Such qualities are too immediate. Moreover, if visual and auditory impressions cannot be cheerful or solemn, how does it happen that another sense department can run the whole gamut of such feelings? Wertheimer in his famous experiments on the phi-phenomenon dealt a fatal blow to one kind of sensory projection by showing that visual movement can exist without any eye movement at all. Koffka and Köhler then took over and by evidence as well as logic made a strong case against empathic projection in perception in general and art in particular—Koffka and his Bryn Mawr lecture on art (68), Köhler in almost everything he has written, but especially in his *Place of Value in a World of Facts* (67). Their point of view, too elaborate to summarize here, can perhaps be indicated by citing the sort of questions at issue. How can a clumsy lout, whose bodily movements are awkward to a degree, ever perceive elegance and grace in the movements of ballet dancers if those qualities must first exist in himself? How can the magnificent outburst of joy in the last movement of Beethoven's *Ninth* ever be heard by those whose subjective hedonic range is incapable of scaling such heights? How can a man of powerful build and aggressive personality ever hope to see the ethereal and pensive charm of Botticelli's women? Indeed, how can any man appreciate the fine qualities of a woman, or vice versa? Such questions tend to answer themselves, according to the Gestalt psychologists: Obviously, something is fundamentally wrong with the doctrine of empathy (94, p. 293 f).

The concept of tertiary qualities, defined and referred to frequently by Koffka and Köhler, presents an entirely different point of view regarding emotion in art and offers a challenge to all students of perception and aesthetics. Tertiary qualities permeate and suffuse all perception, and in art reveal a heightened expression which becomes the very essence of artistic enjoyment and appreciation. Writers who have been influenced by this newer outlook tend to agree on at least three points: (a) Tertiary qualities can only be described by words which also connote subjective moods, but they themselves are not subjective; (b) they are intrinsic properties of visual and auditory perception, not borrowed from any other modality; and (c) they are probably correlated with higher-order stimulus variables. This last conviction awaits proof. If some sort of Gibsonian global psychophysics can eventually produce it, the demise of empathy, at least in the fine arts, will be unavoidable.

Literature that comes at aesthetics from this new angle, although by no means voluminous, is making an important place for itself both in philosophy and psychology. The philosopher Prall, apparently uninfluenced by *Gestalttheorie*, developed a view closely related to the doctrine of tertiary qualities (91, 92). For him, surface appearance is the quintessence of

art, as contrasted with everyday perception that ordinarily leads so quickly and directly into cognition. Surface appearance is the given immediacy of sensuous perception, the unique gift of art to those who are able and willing to let the eyes and ears dwell upon and explore the qualities of surface. Santayana's famous distinction between the first and second terms (100), which for several decades held a dominant place in aesthetic theory, has suffered the same fate as its close relative, empathy. The first term is surface, the object created by the artist. The second term is made up of the pleasures, feelings, emotions, and associations contributed to the object by the subject. The second term fuses with the first. Beauty is pleasure regarded as the quality of a thing. One of the reasons for the strong reaction against this view, and others like it, is that it robs the artist of his due, for it implies that the striking qualities of art have their origin in the viscera of the subject. It is the latter who creates the masterpieces which the artist in his innocence or vanity assumed he had contrived.

The effort to give back to art, as an object of perception, the qualities which common sense would insist the artist himself must have created resembles in several important respects the design for psychophysical research in perception which Gibson has elaborated and carried out in a number of directions (37). The majority view has always been that objects of perception derive their characteristics from some kind of unconscious inference. Their appearance is largely the result of subjective enrichment. Lipps, for example, applied his doctrine of *Einfühlung* to perception as well as to aesthetics. Gibson, on the other hand, has presented a strong case for the view that size, distance, shape, motion, slant, contours, the constancies, and what not, are stimulus-bound in the sense that they are functionally correlated with stimulus variables and thus require no explanation by the assignment to them of processes drawn from the inner and past experience of the subject. The world appears as it does because it is made the way it is. (This statement is of course psychological, not metaphysical. What the world is really like, God only knows.) By the same token, the surface appearance of art results largely from the manner in which artists have manipulated colors and tones. Gibson has made a number of studies, extremely valuable to artists as well as to psychologists, of the way in which two-dimensional cues (pictures and paintings) produce perspective (40, 43); and he has taken a bold stand in aesthetic theory by suggesting that physiognomic (tertiary) qualities are the product of sensory stimulation. We judge the motives and emotions of people by the way they act. What we see are the mobile changes of the surfaces of their bodies, especially the facial surfaces. The subtleties that specify benevolence, for example, are there to be discriminated whether the observer perceives them or not (42, p. 484 f.). It may take a long time to perceive the

blissful serenity in the faces of Fra Angelico's madonnas, but it has been there all the time. Only in recent decades have large numbers of listeners been able to hear the melancholy disillusion that runs through so much of Mozart's music, but it surely must have been there when Mozart put the notes together nearly two centuries ago (41).

The most detailed account of the implications of *Gestalttheorie* for visual art is given by Arnheim in several of his papers (2, 3, 5, 6) and especially in his *Art and Visual Perception* (4)—a book filled with many illustrations and experimental observations bearing upon problems of balance, shape, form, space, light, color, etc. Arnheim's great interest and erudition in art, combined with the factual material he has assembled, invest his theory of expression with a persuasive power which has made itself felt both in philosophy and psychology. In older aesthetic theory, expression was almost invariably treated as a special kind of meaning, a context added to the perceptual core. The work of art was said to give expression to something beyond itself. Arnheim does not deny the possibility of this kind of expression, but he insists that the most significant forms of artistic expression are those which belong to the work of art itself. This statement is an oversimplification of Arnheim's views. He devotes a good deal of space to the symbols and meanings of art, but in keeping with the subtitle of his book, he treats them more as products of the creative eye than as associations that have wandered away from their source.

Some psychologists have begun to wonder whether it is not a prejudice to believe that the primary reaction to the environment consists in the registration of what Heinz Werner has called the "geometric-technical" qualities of sensory data. . . . The face of a person is much more easily and frequently remembered as being alert, clever, energetic, than as being triangularly shaped, having slanted eyebrows, straight lips, etc. . . . a piece of music as soft, nostalgic, dreamy. . . . Expression is the primary content of sensory experiences (2, p. 106 f.).

By bringing percept and concept nearer together, Arnheim has introduced more semblance of order into a confused region. In respect of the point of view that seeks to locate expressive moods within the forms of perception, the philosopher, Martin, has said that "by providing expressionism with a cogent alternative to accidental associationalism, formalism has offered a solution to the knottiest problem in expressionism. This is ironic and reassuring" (78, p. 99).

The method of correct matchings (22) has been used in a number of instances to find out whether subjects can identify tertiary qualities. Works of art and descriptive adjectives, either chosen by expert judges or drawn from Farnsworth's revision of the Hevner adjective list (28), are presented in some sort of random order to subjects whose task is to pick out the adjective that best describes the mood of each object. In one experiment (93), 227 subjects listened to passages from Brahms (stately), Mendelssohn

(sprightly), Mozart (wistful), and Tchaikowski (vigorous) and made correct matchings well over 90 per cent in every case. If the high percentage of correct identifications were the result of empathic projection, it would have to be assumed that the young college students, most of whom were probably bored with their task, felt now a little bit wistful, now a little sprightly, etc., and that they had those incipient moods at just the right times.

Music used in such experiments is usually written in a familiar idiom of Western culture, and the objection has been made that the subjects know the kind of verbal labels scholars and critics have attached to the passages chosen. In order to examine this objection, Brown (18) did a matching experiment in which cacophonous and presumably unfamiliar passages by contemporary composers (Hindemith, Berg, Stravinsky, and Bartok) were listened to by 113 subjects. The correct matchings ranged from 70 per cent to 97 per cent, a bit lower than for familiar music, but all of them way above chance. Music is a complex perception. Styles may change, but the ear can apparently detect in their varieties certain groupings of tones that are coercive in presenting the tertiary qualities of mood. As modern music becomes more familiar, differentiation and specification are easier and more accurate, as Welleck (116) and Mull (86) have shown, and everyday experience tends to confirm.

Modern abstract paintings are baffling to many observers, and one might suppose that matching them to descriptive adjectives would not exceed chance. An experiment by Porter (90), however, indicates that the tertiary qualities of abstract paintings can be identified as easily as in music, which of course is also abstract in the sense that it portrays no object. With the help of an artist and expert judges in an art department, Porter had placed at his disposal six colored abstract designs and six adjectives which the judges agreed fittingly described the respective moods of the designs. The designs and adjectives were presented in random order to 117 subjects. The matchings were far above chance, ranging from 66 per cent to one design correctly matched by every subject. Color seems to enhance the salient characteristics of abstract designs and figures, as a study by Swan (105) using some of the random shapes devised by Attneave (11) has shown.<sup>6</sup>

\* It goes almost without saying that in the enjoyment of art the tertiary qualities ordinarily do not have verbal labels attached to them, *Gottseidank*. They are, nevertheless, there for the eyes or ears to absorb namelessly. There is a growing conviction that their production by artists, who also rarely name them and probably are unaware of what they are doing in this respect, is the very quintessence of art. Beauty is becoming an outmoded topic in aesthetics, partly because so much recent art portrays moods that are anything but lovely, although often powerful in their expressiveness.

The phrase "tertiary qualities" has not come into wide circulation, but the idea in back of it—the expressiveness of art as a property closely bound up with the perceptual structure—has become a prominent theme in present-day aesthetic theory. The French psychologist, Francès, has brought together in his *La perception de la musique* (35) the results of many years of devoted labor and thought in the fields of acoustics, auditory perception, and aesthetics. His writing is encyclopedic and his point of view eclectic. He devotes several chapters to an impartial examination of the various answers given to the question of musical meaning. He recognizes the role of association and context in this matter, but his own inclination, expressed with Gallic regard to the amenities of dispute, is to look upon the musical percept as an end in itself rather than a point of departure for nonauditory reverie.

L'expressivité apparaît comme la donnée brute inhérente à la structure (p. 272) . . . la musique est langage et, en tant que telle, vaut par ce qu'elle dit, par la manière dont elle le dit . . . elle exprime au moins une manière d'être, un être, ou un drame, une impression vécue. Alors même que l'auditeur est très éloigné de la chercher, cette expressivité le saisit sans qu'il s'en doute, sans qu'il la formule verbalement. R. Arnheim a eu raison de souligner la priorité dans le temps de ce sentiment qui accompagne l'audition (p. 384).

Welleck, the German psychologist whose contributions to *Musikwissenschaft* have gained wide recognition, is more emphatic and dogmatic in stating his position (115). The dichotomy between percept (formalism) and expression disappears in *Ganzheitspsychologie*. Mood and feeling are part of the perceptual whole. They are diffuse and difficult to describe, but they express in external form the innermost nature of man. The genius of the artist makes articulate what the ordinary man can hardly express even to himself, let alone to anyone else.

Alle Form ist ausdruckschaft, aller künstlerischer Ausdruck geformt (p. 688). Die Lösung der Kontroverse zwischen Formal- und Ausdrucksästhetik liegt in einer Synthese: einer Form-Ausdruck-Ästhetik, die freilich noch geschrieben werden muss (p. 684).

The phenomenon of expressive form is by no means confined to art. All objects are expressive to some degree, although as a rule less palpably so than in art because the practical business of everyday life can rarely afford the luxury of detached contemplation. Yet a driver in heavy traffic may catch a glimpse of the graceful and soaring lines of a church steeple—and also of the nasty look on the face of a pedestrian whom he just managed to miss. German psychologists still seem more willing than their American colleagues to explore the implications and variations of *verstehende Psychologie* as originally expounded by Dilthey, Spranger, and Jaspers. Their

work overlaps and often includes aesthetics proper, although most of their studies deal with physiognomic perception in everyday life; but their point of view is similar to a number of others which are bringing different interests into the same universe of discourse.

Lersch seems to be the most prolific and frequently quoted writer in contemporary German physiognomic psychology. His large volume, *Aufbau der Person* (72), which is now in its seventh revised edition, gives the reader a massive amount of material to digest. Everything an individual does can be made an object of phenomenological scrutiny—his movements, gestures, postures, pantomime, *Mimik* (apparently movements that are expressive, although constant repetition has made them more or less inflexible and stereotyped), gait, carriage, quality of voice, and especially the movements of the face and eyes. These presentations are not measured as Sheldon would measure body types. They are judged with respect to their physiognomic disclosures: lively, depressed, languid, gay, cautious, determined, indecisive, petulant, sympathetic, timid, aggressive, and so on. Personality is in the first instance an external manifestation and can be observed in exactly the same way as one would examine a work of art. Whether the physiognomic qualities correspond to or reveal a person behind the surface is another matter, one which obviously is of vital importance to students of personality. Fortunately, aesthetics need not bother with that problem. A work of art has no *innere Seele*, only a surface; but the concern of physiognomy with surface appearance as the first step toward an understanding of personality is all of a piece with the exploration of aesthetic surface. The two fields can therefore borrow methods (greatly in need of improvement) and findings from each other.

A more readable and engaging book by Lersch is his *Gesicht und Seele* (73). A large number of pictures taken from films show in minute detail the parts of the face which give rise to high agreement in judging various expressive qualities. Herland (52) has furnished a veritable lexicon for facial expressions. But physiognomic perception is not confined to the study of persons and animals. "Ausdruck bezeichnet alle jenen physischen Erscheinungen in denen sich psychische Vorgänge und Zustände spiegeln" (113, p. 192). The whole perceptual world is alive with physiognomic qualities, and the student of aesthetics will find that their exploration by German psychologists has opened up large territories of valuable information, of varying degrees of statistical reliability, which is patently relevant to the philosophy and psychology of art (16, 20, 48, 54, 79, 99, 104).

The acknowledged master in the study of physiognomic perception in this country is, of course, Heinz Werner. He introduced the phrase and the concept into experimental psychology some 35 years ago, and has been extending the range of application ever since. In his book on mental develop-



ment (117), a revision and reworking done in 1940 of a volume which had become a classic on the Continent, Werner devotes a number of sections, carefully listed in the index under physiognomy, to those qualities of behavior, especially in children, which the usual methods of analysis, because of their fixation on elements and parts, fail to capture. Werner's clearest and most illuminating account of physiognomic perception, especially in relation to art, is presented in a brief section in Kepes' *The New Landscape in Art and Science* (121).

Werner's students and associates at Clark University have published a large number of studies (e.g., 23, 24, 66, 110, 111, 112, 120) designed to measure the dynamic as opposed to the structural properties of a perceptual object. In reversible figures, for example—especially those that contain human faces—each reversal produces a structural change which also frequently reveals a strong directional shift, a dynamic thrust to the right or left (119, p. 52). The apparent direction of autokinetic motion is markedly influenced by ascending and descending tonal glissandi (82). The apparent location of the median plane of an object or picture is significantly shifted toward the center of a figure placed asymmetrically with respect to the objective median plane (109, 118). Innumerable dynamic properties of this sort are presented all the time in everyday perception,<sup>7</sup> but for the most part their description has remained at a phenomenological level without benefit of statistical and laboratory support. The studies done at Clark make use of careful experimental design and statistical procedures, and also reveal ingenuity in the construction of apparatus and stimuli that make palpable to the observer whatever phenomenon is to be investigated and measured.

A phenomenon in which physiognomic and tertiary qualities play a fascinating dual role is that of metaphor, a subject which Asch has of late been exploring in some detail. Asch was led to pursue the topic in various directions after he noted that a change of one word in two otherwise identical lists of adjectives drastically altered the impression formed of a person by two groups of subjects. The insertion of "cold" in one list and "warm" in the other made all the difference (8). It is difficult to find in English an adjective that does not apply equally well to both physical and

<sup>7</sup> These properties have not escaped the sharp eyes and ears of the artist. Architects, musicians, sculptors, and painters constantly talk about balance, proportion, harmony, symmetry and asymmetry, etc. What they say often sounds like babel-babble, but in their work it is obvious that the best of them know very well how to arrange colors, shapes, distances, tones, chords, tempos, and what not, in such a way as to achieve exactly the dynamic effects they want. Before long, new chapters in experimental psychology will surely be able to specify the stimulus variables for some of these effects and will also resolve for a season some of the ancient controversies in aesthetic theory.



psychological events. People are deep or shallow, narrow or wide, bright or dull, hard or soft. Things and people are warm, cold, hot, pale, straight, twisted, crooked, sweet, bitter, colorful, rough, smooth, slippery, dry, cloudy, broad, and so on (9). Are these dual functions the result of fortuitous association? Asch is strongly inclined to answer that question in the negative, both on systematic grounds formulated earlier in the chapter on the expression of emotion in his *Social Psychology* (7) and on the basis of many metaphors of almost exactly the same dual function and meaning which he has found in several widely different languages such as ancient Hebrew and Greek, Chinese, Thai, Malayalam, Hausa, and Burmese.<sup>8</sup>

The words "express," "expressive," and "expression" are used in aesthetic theory in many different ways, as Rudner has been at some pains to point out (98). Since the words will presumably remain in the literature for a long time, the best that can be done by the writer who uses them is to define as carefully as possible what he means, and let it go at that. The most difficult meaning is the one that pervades many of the studies cited in the preceding paragraphs, namely, the growing tendency in experimental aesthetics, and also in philosophy, to regard expression as an intrinsic quality of perception closely resembling mood and emotion (21). This view almost cries out for an iconic (69; 83, p. 193; 70, p. 27) or isomorphic interpretation. Art forms are isomorphic with the forms of feeling and mood. Unfortunately this highly plausible interpretation, and the one which would give to aesthetics an almost miraculous solution to the ancient riddle of how emotion finds its way into art, is incapable of proof in the absence of an acceptable phenomenology of emotion. It is high time, nearly three-quarters of a century after James' famous chapter, to find out what emotions feel like. To say, for example, that music sounds the way emotions feel is hardly more than a play on words. Block's study of emotion (15) by the application of Osgood's semantic differential is an impressive effort to turn play into work, and may pave the way to the second great chapter on emotion. In the meantime, psychophysics is more profitable than isomorphics.

Scholars in the various arts have been prolific to a staggering degree in their historical and analytical studies, but they approach their subject from an angle that ordinarily offers little direct assistance to the psychologist. There have recently been one or two outstanding exceptions. Meyer in his *Emotion and Meaning in Music* (80) combines musical scholarship with an expert knowledge of relevant psychological literature. The result has been hailed as a contribution of the first order. The bulk of Meyer's book deals with perception of pattern, a central theme with many variations illustrated

<sup>8</sup> By the time this volume comes off the press, a study made by Asch on the development of metaphor in the language of children will have appeared in *Perspectives in Psychological Theory: Essays in Honor of Heins Werner* to be published by the International Universities Press, New York, N. Y.

by examples from music chosen to test the principles used by Gestalt psychologists in their treatment of visual perception. In a later article (81) Meyer draws upon information theory to distinguish two kinds of musical meaning: (a) embodied meaning, which is iconic with the sensory-perceptual material, and (b) designative meaning, which refers to something beyond the material given in perception. The English music critic, Howes, has tried to clarify what is meant by music as the language of emotion (55), but his brilliant essay is confused, although not more so than the psychological literature which he has obviously studied far beyond the call of duty as a critic.

Gombrich, the eminent art historian and director of the Warburg Institute in the University of London, has expanded his Mellon lectures given at the National Gallery of Art in Washington into a book, handsomely printed and illustrated, on *Art and Illusion* (45). His knowledge of relevant literature in psychology is also extensive, but, again like the literature itself, it is frequently ambiguous and inconsistent. The thesis that Gombrich seems to defend is that art mirrors not the external environment but rather the inner life of man, a very ancient doctrine indeed, but one which Gombrich tries to make less puzzling by his insistence that the inner life is mirrored in the visual material itself.

The growing awareness that art offers a key to the mind . . . has led to a radical change of interest on the part of artists. . . . The language of forms and colors . . . that explores the inner recesses of the mind has come to be looked upon as being right by nature. Our nature (45, p. 360).

Our nature is presented in visual form. Art is a matter of optics, a training of the eye.<sup>9</sup> He who constantly looks for something more than is given him by the artist runs the danger of losing his optical virginity, which, as someone remarked, when once lost is like any other virginity, gone forever. A recent article on physiognomic perception (46) would seem to indicate that Gombrich attaches primary importance to the intrinsic expressive quality of visual form.

If more scholars in the humanities are beginning to welcome assistance from their scientific colleagues, no small amount of credit for the change in attitude must go to Thomas Munro, curator of education in the Cleveland Museum of Art, founder of the American Society for Aesthetics, and editor of the *Journal of Aesthetics and Art Criticism*. His book *Toward Science in Aesthetics* (87) is the latest in a long series of publications in which he has urged more extensive application and liberal acceptance of scientific method in the analysis of art.

\*One is reminded of Cézanne's remark about Monet: "Monet n'est qu'un oeil—mais quel oeil!" Gibson would probably speak of the eye that has learned to discriminate what is there.

The formal or fine arts lend themselves to scientific analysis, although not always of the experimental or laboratory variety (e.g., 1, 26, 38, 114, 122) more readily than do the verbal arts. As a rule the latter clearly involve a greater admixture of what Santayana called the second term, a large domain of psychological experience difficult to get at except from the philosopher's sturdy old armchair, many of whose newer occupants keep psychoanalytic literature within arm's reach (33, 34, 53). Art as an escape or wish fulfillment has now become a topic too trite even for cocktail conversation, but the wise and learned philosopher, Parker, developed that theme many years ago in his *Analysis of Art* (88) with such good taste, logic, and scholarly restraint that the book has held a high place in aesthetic literature ever since—and today his thesis is less incompatible with formalism than it once seemed to be. The notion that art is imaginative fulfillment of wish—by no means always sexual, and certainly not an escape—contains what many regard as a profound truth, although hardly capable yet of empirical verification, and may account for the fact that at all times and in all places art has had a more tenacious hold over the mind of man than any other human achievement. Art is completion of unrealized hope.

The role of attitude in the aesthetic judgment has been investigated from almost every conceivable angle. A good many studies reflect the influence of the numerous and far-reaching implications of Bullough's notable essay on psychical distance, first published in 1912 and now available in book form along with several of the author's later lectures (19). Preoccupation with the practical consequences of action is almost certain to produce underdistance in relation to the qualities of an object, while overdistance results from lack of any interest at all in the object. Aesthetic contemplation occupies a region somewhere between the two extremes, a region in which the observer's attitude is characterized by an interested and yet detached concern for exploring the qualities of surface. Since proper psychical distance is shifted or disrupted by all sorts of factors, experimental aesthetics for many years has been on the lookout for those factors, and the search still goes on. Factor analysis has rediscovered a number of them (47). Das, Rath & Das (25) have followed Sherif in studying the effects of prestige and suggestion. Asthana (10) has found that changes in preference are brought about by altered instructions. Lawlor (74) has been able to show that group discussion of paintings makes for greater agreement in rank orders. And the philosopher, Morris, by the use of the newest scaling methods has found some relation between preferences and Sheldon's body types (84, 85). These studies, and others in similar vein, deserve far more consideration and comment, but space is running out.

In spite of variability of attitude in aesthetic judgments, there is increasing evidence that *De gustibus non est disputandum* needs considerable

revision. Farnsworth is the undisputed authority regarding musical taste in this country and probably in every country where Western music is known and heard. By unflagging effort he has accumulated an astounding amount of material that testifies to an almost unbelievable uniformity of rank orders of composers derived from musicologists, practicing musicians, college and high school students, concert programs, histories of music, encyclopedias, etc. (27, 29). "Is musical taste a matter of whimsey or is it in some way lawful? The answer is clear—taste is lawful" (29, p. 152).

Sopchak (101) asked 500 college students to assign affective values to each of 15 pieces of music. Six weeks later the test was repeated and yielded a retest reliability of .76. Sadacca (quoted in 95, p. 6 f.) presented abstract designs and pairs of colors to 178 subjects by the method of paired comparisons and obtained 70,488 judgments of preference. If B is preferred to A, and C to B, then, presumably, C will be preferred to A—except perhaps in aesthetics. If C is not preferred to A, such a reversal is called a circular triad. In Sadacca's experiment 37 triads could occur by chance in each series of judgments. The actual number was only 6.25. This amazing consistency of preference would seem to call in question the frequent assertion that there is no disputing about tastes. Aesthetics needs many more studies along these lines, as well as investigations of cross-cultural preferences.

Opinion is widespread that cultural factors play a decisive role in aesthetic preference. Yet there is some evidence that this opinion is also in need of revision. Feelings and moods must be pretty much the same the world over. If so, then their expression in outward form would presumably have elements in common across cultures, however much obscured at first by stereotypes and peculiarities of time and place. Morris (85) secured ratings of modern paintings from various groups of college students in this country, and then presented some of the same paintings to groups in China and India. In spite of several marked differences in preference, there was a comfortable margin of significant agreement in the three countries. Lawlor (75) presented eight West African designs to 56 English students and to 56 West Africans. Within each cultural group the subjects showed high agreement. The cross-cultural agreement was lower and equivocal in relation to relative influences of culture and design-patterns. Jahoda's study (63) of 858 young people in the middle schools of Accra, West Africa, was concerned primarily with the influence of sex symbols in the choice of pictures. The same pictures had been rated by children in Scotland. The marked differences in the position allotted to some of the pictures were thought by Jahoda to be the result of less inhibited sexual activity among the young Africans. Voorhees (108) managed to find a musicologist who had studied the moods represented by Bedouin, Indonesian, Javanese, Korean, and Maori folk music. Ten typical examples and 10 adjectives were

presented to 73 American undergraduates for matching. The outlandish sounds caused both laughter and aversion, yet the correct matchings ranged from 77 per cent to a high of 99 per cent for a Bedouin piece of music that was said to have something to do with love. These studies are too few for any weighty conclusion, but they suggest that cultural relativism has gone a bit too far. Stephen Pepper, whose writings on the philosophy of art occupy a place of high eminence, has recently given critical thought to this thorny question (89, especially pp. 122-151).

It has often been said that aesthetics is a field of inquiry in search of a method. One may hope that psychometrics (49, 50, 51, 107) and the newer psychophysics (39) will soon make that statement out of date, if they have not done so already. The psychophysical procedures would certainly seem to be the better way of dealing with the formal arts (i.e., those which have some functional dependence on stimulus-variables) whereas the scaling methods could be reserved for aesthetic values which fail to turn up stimulus-correlates. Thurstone's statement that the aesthetic value of an object is determined entirely by what goes on in the mind of the perceiver and that it would be well-nigh hopeless to look for stimulus-correlates (106, p. 193) is questionable and certainly far too pessimistic.<sup>10</sup> In any event, there are two kinds of aesthetic value, and the two procedures nicely supplement each other.

Discriminatory reaction on the part of human observers is, of course, accepted as the fundamental operation for exploring aesthetic experience, but, in the construction of theory, reaction and experience are by no means regarded as interchangeable items. Methodological behaviorism and behaviorism as a definition of subject matter are not the same thing. The latter has had no influence at all in aesthetics, with one striking exception (76, 77). Students of aesthetics think of their subject matter as affective experience full of colors and words and sounds molded in such a manner

<sup>10</sup> Scaling methods are used with advantage in attempts to assign quantitative relations to subjective experiences. The meaning of the word "subjective" is almost always likely to be unclear unless carefully defined. In the present context it apparently refers to psychological events for which stimulus correlates are lacking or unknown. Values are often called subjective, but values are also facts, and whether or not they are stimulus-bound is an important matter to find out. All values, all facts, all experiences, no matter in what branch of science they may happen to fall, start life as private events but not necessarily subjective. Stars and molecules cannot report themselves. Someone has to observe them (a private experience) and then report what he has seen (a public document). Private events enter the public domain by the lucky circumstance of language, but they are not for that reason necessarily objective. To what extent aesthetic values are objective, subjective, or a combination of both is a question for experimental psychology to answer without prejudging the issue by assuming that all values are subjective.

as to show forth an almost inexhaustible galaxy of moods. This naive attitude, if such it be, will almost certainly never be replaced by one in which the pressing of a key, or any other kind of response, is made equivalent to the phenomena of art.

Plato's formalism has inspired many scholars to attempt a mathematics of art. The time may come, if psychometrics and psychophysics succeed in their task, when the *Aesthetic Measure* (14) of the late George Birkhoff, Harvard's illustrious mathematician, will gain the recognition which during the lifetime of the author it failed to receive.

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## PERSONALITY STRUCTURE<sup>1,2,3</sup>

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In line with the master plan for future *Annual Reviews of Psychology* outlined in Volume 11 of this series, the present survey of theory and research in personality has been given the restrictive title of "Personality Structure." In many ways the present review complements Atkinson's (7) treatment of "Personality Dynamics" in the previous volume, both in the time periods covered and in the material allocated to the general domain of personality structure in the present attempt to delimit that topic in contrast to dynamics.

In the initial volume of this series, Sears (174) distinguished three main categories for summarizing research in personality—development, dynamics, and structure. He characterized structure as the analysis of substantive components of personality. Since the term substantive does not have a clear reference in the present context, however, a more explicit delineation is sought. If personality dynamics is considered to emphasize drives, motives, and the interplay of forces, it is tempting, particularly for dynamic psychologists who wish to capitalize upon semantic overtones, to follow a gross physical analogy and define personality structures as being static, passive, and lacking in force. However, another, perhaps ancillary, aspect of dynamics, an aspect reflecting change and momentary states, was repeatedly stressed by Atkinson in multiple references to "contemporary" dynamics, "momentary environmental influences," and "momentary situational factors." In contrast to this emphasis in personality dynamics upon

<sup>1</sup>Since the survey of personality literature in the previous volume of the *Annual Review of Psychology* was devoted entirely to personality dynamics, the present review of personality structure covers the two-year period from May, 1958 to May, 1960. A contemplated section in the present chapter on "Pathological Structures" was eliminated to avoid duplication with the chapter on "Classification of Behavior Disorders."

<sup>2</sup>Abbreviations used in this chapter include: E and N (Extraversion and Neuroticism Scales from the MPI); EFT (Witkin's Embedded-Figures Test); EPPS (Edwards Personal Preference Schedule); FC and CC (Flexible and Constricted Control on the Stroop Color-Word Test); F Scale (California Authoritarianism Scale); MMPI (Minnesota Multiphasic Personality Inventory); MPI (Maudsley Personality Inventory); OAS (Over-all agreement score); PF (Cattell's 16 Personality Factor Questionnaire); RFT (Witkin's Rod-and-Frame Test); SDS (Edwards' Social Desirability Scale); SVA (spiral visual aftereffect); TAT (Thematic Apperception Test); WAIS (Wechsler Adult Intelligence Scale); WJT (Luchins' Water-Jar Test).

<sup>3</sup>The author wishes to thank Douglas N. Jackson, Nathan Kogan, and John Ross for their many helpful and clarifying comments on the manuscript and Ruth B. Ekstrom for her extensive bibliographic assistance.

changing momentary states, the conception of personality structure employed in the present chapter gives prominence to stability and reflects many of the standard properties of general mental structure (71) as applied in the personality domain: Personality structures are stable, relatively enduring components of personality organization that are invoked to account for recurrent similarities and consistencies in behavior over time and over situations.

In his recent discourse on psychoanalytic theory, Rapaport (160) has pointed out that "structural determiners of behavior were introduced as intervening variables to account for the observation that motivations do not determine behavior in a one-to-one fashion" and that in contrast to drive processes, "whose rate of change is fast and whose course is paroxysmal," structural factors appear relatively permanent, or at least of a slower rate of change. Gill (98) has also referred to structure as "that aspect of the mental process which characterizes its place in the enduring, stable organization of the mind," including "steady, stable, ordinary, organized, enduring patterns of behavior and thinking."

The organization of the present review is based partly upon categories which emerged in attempts to classify the particular set of papers covered and partly upon the reviewer's current conceptions about the nature of personality structure. The organization was also influenced by the appearance of Guilford's book on *Personality* (106), with its explicit distinctions among dimensions of aptitude, temperament, motivation, and pathology. An attempt will be made to treat the topic broadly and to include several levels of personality organization from the surface patterning of behavior and the specific components inferred to account for such consistencies to superordinate constructs, such as ego and self, which involve hierarchical organizations of many specific structures. The particular categories used, however, are intended to represent only one convenient way of dividing the domain under consideration; they are considered to be neither mutually exclusive nor exhaustive of the topic. The fact that one section, for example, is called "Perceptual-Cognitive Structures" and another "Stylistic Structures" does not imply that cognitive or stylistic components are not important influences in studies reviewed in other sections; rather, the titles are intended only to reflect relative emphases in the discussion.

#### DIMENSIONS OF INTELLECT

In attempting to classify some 50 distinct intellectual abilities that have been isolated factor analytically, Guilford (105, 106) has emphasized resemblances among factors in terms of the psychological processes or operations required, the kinds of test materials or contents used, and the kinds of products involved when certain operations are applied to certain contents. He has distinguished (a) five processes—memory, cognition, divergent thinking, convergent production, and evaluation; (b) four types of material with figural, symbolic, semantic, and, tentatively, behavioral content; and

(c) six products involving units, classes, relations, systems, transformations, and implications. If a factor is predicted for every cell of this three-way classification system, which has been proposed as a model for the structure of intellect, then 120 different abilities would be expected and, from Guilford's viewpoint, should be actively sought experimentally. This model is far removed from the notion of a single dimension of general intelligence and its attendant requirements to account for all too many sources of variance (137). Guilford's model represents a logical system based upon a conceptual analysis of perceived similarities among factors. A more empirically based theory of intellect might well be derived, however, if this cubic model were complemented and modified by psychologists without Guilford's predilection for orthogonal factors, who would utilize, albeit with a gingerly evaluation of sampling influences, the consistent empirical interrelations among factors reflected in oblique simple structures.

Saunders (167), in a factor analysis in which each subtest of the Wechsler Adult Intelligence Scale (WAIS) was represented by two independent scores, provided evidence for the statistical significance of 10 orthogonal dimensions for the WAIS, although the factors did not correspond perfectly with the subtests. This demonstration of reliable unique variance for certain WAIS subscales might be taken as support for the widespread clinical use of difference scores and profiles, but the interpretation of such composites, whether in terms of differential abilities or personality patterns, is made considerably more complex by Saunders' (168, 169) subsequent finding that items within various subtests do not reflect single common processes. Subscales of the Wechsler-Bellevue were also factor analyzed by Mundy-Castle (146) in a battery that included measures of alpha index and mean alpha frequency from electroencephalograms. His conclusion that three of the factors—a visual-concrete aspect of intelligence, a verbal-abstract aspect, and a temperamental-excitability aspect—also involve non-intellective electrophysiological components is considerably attenuated, however, by the presence of three dependent IQ indices in his matrix. Wechsler IQ and subscale scores, in particular performances on digit span, were interpreted in terms of inhibition processes in ego functioning by Spivack, Levine & Sprigle (185). The significant correlations they obtained between IQ and three measures presumed to reflect delay functions of the ego—time estimation, the Stroop Color-Word Test, and Rorschach human movement responses (133)—were used to support the feasibility of treating both intellectual and personality variables within the single theoretical framework of ego psychology (160).

*Flexibility and rigidity.*—In attempting to clarify the nature of the two types of thinking-flexibility obtained in previous factor analyses, Guilford and his co-workers (91) analyzed a large battery of tests selected to permit an evaluation of the hypothesis that one form of rigidity, perseveration, is the opposite of spontaneous flexibility, and another form of rigidity, persistence, is opposite to adaptive flexibility. The two usual flexibility fac-



tors were identified in the analysis with some of the tests designed to measure perseveration receiving significant loadings on spontaneous flexibility and some tests of persistence receiving significant loadings on adaptive flexibility, both in the appropriate direction. Since, in addition, no evidence was uncovered to support separate dimensions of perseveration, persistence, or rigidity, the general hypothesis received support. In light of these results, the conception of spontaneous flexibility was revised to represent a disposition of freedom from inertia in thinking, and adaptive flexibility was considered to involve a restructuring of interpretations and approaches in problem solving and elsewhere. An important ancillary finding in this study was that an adaptation of Luchins' Water Jar Test (WJT) bore almost no relationship to either of the flexibility factors but received high loadings on general reasoning and logical evaluation.

Levitt & Zuckerman (134), in reviewing research on the Water Jar Test and its correlates, also pointed to evidence that WJT measured intellectual factors and suggested that most correlations and factor loadings used to support its construct validity as a measure of rigidity would probably disappear if intellectual components were adequately controlled. In view of the prevalent negative evidence on the validity of WJT (134), such findings as Ainsworth's (3)—that pre-examination stress tends to produce rigidity in problem solving and that less acculturated people are more rigid—should probably not be generalized beyond the particular test performance.

Chown (51), in reviewing the concept of rigidity, its operational measures, and interrelationships among them, concluded that each test at present must be treated separately. Overlap between studies with any of the 47 rigidity tests reviewed was found to be extremely small. When two experimenters do use the same tests, however, "their results hardly ever agree and it is hard to say whether this is due to faults in the tests or discrepancies in the conditions, administration, and scoring." The effect of *Einstellung* has also been extensively examined by Luchins & Luchins (139) in a detailed compilation of theoretical formulations and research findings dealing with rigidity of behavior.

Fink (87) reported negative evidence on the generality of rigidity and reinforced the position that the concept is too complex and its measures too poorly defined to expect consistent effects. On the other hand, Philip (155), in a correlational study unfortunately attenuated by its sample size of 28, uncovered an interrelated cluster of three laboratory measures of perceptual rigidity and six relevant trait-rating scales. Belmont & Birch (12), while also noting little consistency of rigid performance from one task to another, did point, however, to a general positive trend in predicting rigidity in various task situations from Rorschach categories, suggesting that the Rorschach's broad range of responses can serve as a basis for general, low-level prediction for a variety of task conditions.



## PERCEPTUAL-COGNITIVE STRUCTURES

The present section deals primarily with consistent individual differences in the capacity to experience certain perceptual phenomena or to perform various perceptual-cognitive tasks, as well as with systematic differences in the nature or content of task responses or perceptual reports.

*Individual differences in perception.*—Barthol (9), in testing the hypothesis that individual differences in kinesthetic figural aftereffects and in the movement-simultaneity threshold of phi-phenomenon are both determined by individual differences in cortical conductivity (128), uncovered what he interpreted to be sex differences in brain structure when the theoretically required significant correlations between the two measures proved to be of opposite sign for the two sexes. Spitz & Blackman (182) reported significant differences between mentally retarded and normal groups in the capacity to satiate on a visual figural aftereffect test and in perceptual rigidity on a reversible figures test. A significant, but possibly spurious, correlation between scores for aftereffects and reversible figures for both groups combined was taken as support for a common factor underlying both perceptual rigidity and limited capacity for satiation. Spivack & Levine (184) observed differences between brain-damaged and normal groups on measures of spiral visual aftereffect (SVA), reversible figures and a visual figural aftereffect, but SVA was found to be unrelated to age, intelligence, memory, reversal rate of reversible figures, and amount of figural aftereffect. Since the latter two measures were also found to be uncorrelated, the over-all results were not in accord with the assumption that a characteristic level of cortical conductivity (128), modifiability (199), or reactive inhibition (74) serves as the chief determinant of individual differences in such phenomena. Gollin & Bradford (100), however, have pointed to the problem of faulty communication in measuring SVA, because some subjects, especially young children and brain-damaged adults, "may be unable to supply the verbal designators necessary for the achievement of a positive score." When verbal descriptions were first elicited for spirals painted on expanding and contracting balloons, subjects considerably younger than previously reported were subsequently able to achieve success under illusory conditions.

Several studies also appeared during the period reviewed attempting to relate individual response differences on perceptual tasks to various personality and temperament measures. Zuckerman & Buss (211), for example, studied prerecognition responsivity and perceptual defense in relation to hostility, anxiety, and impulsivity, and found psychiatric patients to be significantly higher than normals in prerecognition responses but not in defensiveness. Although defensive tendencies appeared to be unrelated to any of the personality measures used, prerecognition responsivity turned out to be negatively correlated with MMPI indices of anxiety, internalization,

and social introversion. Davis (60) utilized differences in the resolution of binocular rivalry in stereoscopic perception to study perceptual defense, and Kohn (131), with an essentially identical procedure, found that patients sensitive to words dealing with aggression, sex, and dependency tended to produce more TAT stories involving conflicts in these areas.

*Memory and attention.*—Two chapters in Schachtel's *Metamorphosis* (170) dealing with memory and with attention are relevant in the present section. In the first, childhood amnesia was considered to occur not merely through repression of specific content, but also because

the biologically, culturally, and socially influenced process of memory organization results in the formation of categories of memory which are not suitable vehicles to receive and reproduce experiences of the quality and intensity typical of early childhood.

In the chapter on attention, Schachtel stressed the directional quality of focal attention and its developmental importance in changing the diffuse awareness of infancy into a state in which distinct needs and feelings are differentiated and discrete objects articulated in the environment. The importance of directed attention in perceptual articulation has also been emphasized by Köhler (129, 130), Piaget, Vinh-Bang & Matalon (156), and others (96, 97), whereas Heider (109), proceeding from an ecological orientation, has stressed the significance of environmental structures and constraints for a comprehensive understanding of perception.

*Cognitive differentiation.*—Several studies have recently appeared which have attempted to analyze the cognitive significance of certain Rorschach developmental indices in terms of developmental levels of cognitive function. Brooks & Phillips (30), as a prerequisite to relating genetic levels of Rorschach performance to stages of cognitive differentiation, categorized several objective tasks in terms of (a) Werner's (197) polar opposites of "rigid-flexible," "syncretic-discrete," and "diffuse-articulate," which together describe a continuum ranging from undifferentiated to hierarchically integrated behavior; and (b) three stages of mental development—sensory-motor, perceptual, and conceptual. When Rorschach indices for 38 subjects were related to performance measures in this double classification system, no consistent patterns appeared between stages of cognitive development and Rorschach scores. However, a preponderance of significant relationships did emerge between low genetic Rorschach indices and the rigid-flexible classification and between high indices and diffuse-articulate or syncretic-discrete measures. A subsequent rational analysis of the results led to the formulation of two new dimensions which appear strikingly similar to Witkin's (207) conception of field independence: (a) a tentative dichotomy of tasks requiring the internal formulation of organizing principles as opposed to the manipulation of the external field, and (b) "an ability to decontextualize an element from a totality and to handle it appropriately."

Lipton, Kaden & Phillips (136), in an attempt to explain relationships among various Rorschach indices and cognitive tasks in terms of these new dimensions, hypothesized two developmentally ordered types of decontextualization and, leaning heavily upon their interpretation of Thurstone's (188) factors of speed and flexibility of closure, classified several performance tests as representatives of either an "articulation" type or an "independence" type. Although, with a sample of 32 cases, the tests within the independence category did not show the significant interrelationships revealed by the articulation type, there was, nevertheless, a tendency for high Rorschach developmental indices to be related to articulation measures and for low or intermediate indices to be related to independence tests. In cluster analyses of tetrachoric intercorrelations among Rorschach indices for two samples of 32 and 37 cases, Podell & Phillips (157) uncovered three clusters, roughly equivalent across samples, which were presumed to reflect the developmentally ordered dimensions of "globality, varied productivity, and accuracy and human movement." Cluster analyses of cognitive tasks, however, revealed four clusters in each sample with only one overlapping. When these seven separate cognitive dimensions were rationally ordered in a presumed developmental sequence and related to the Rorschach developmental ordering, the lower ends of both scales displayed moderate agreement, but the two higher Rorschach levels were reversed with respect to the higher dimensions of cognitive tasks. Feffer (83) reported a slight relationship between two assessments of cognitive developmental level, one in terms of genetic Rorschach indices and one in terms of a formal analysis of role-taking performance. Smith & Phillips (178) explored possible relationships between Rorschach developmental levels and effectiveness of social adaptation, with equivocal results.

It seems to the reviewer that this series of papers on differential cognitive structures is important in conception but quite limited in contribution, primarily because of the cautiousness required in generalizing from relational patterns based upon such small samples. Since relationships were also usually reported (30, 136) only in terms of significance levels from chi-square contingency tables, magnitudes of correlations were not available to evaluate interpretations. Provided that adequate provision could be made for psychometric pitfalls in Rorschach scores (111), factor analysis could also have been profitably applied in this instance, not only to appraise the initial hypotheses, but to uncover empirical dimensions of cognitive functioning and Rorschach response with which to short-cut the successive *post hoc* formulations and appraisals so tortuously presented in these papers. More important however, an appropriate application of factor analytic design would have made explicit the dangers inherent in seeking dimensional representations with small samples, since the number of possible dimensions reflected in a particular set of data is limited by the size of the sample as well as by the size of the test battery.

*Interpersonal perception and social judgment.*—The rather limited intention of the present section is to summarize briefly some studies dealing with social perception in relation to personality organization; no attempt will be made to treat the processes of interpersonal perception and social judgment per se. Emphasis will be given to articles relating the perception of social stimuli to characteristics of the perceiver, rather than to those utilizing perceived stimulus variations to differentiate among social objects.

Bronfenbrenner, Harding & Gallwey (27), in reaction to a widespread interpretation of skill in social perception as a single generalized ability to judge other people, distinguished between two major types of ability in this area—sensitivity to the social norm or generalized other and sensitivity to individual differences. As characteristics of individual judges, these two component skills were found to be essentially uncorrelated and to be differentially associated with ratings of various personality traits. Cline & Richards (52) also presented evidence that the general ability to perceive others accurately, which was apparently reflected in substantial intercorrelations among several judgmental measures, could be partitioned into two independent parts interpretable in terms of Bronfenbrenner's distinction.

Altrocchi (5), in attempting to evaluate the hypotheses (a) that people choose to interact with others complementary to themselves in dominance and (b) that dominant people perceive others as being less dominant than do submissive people, found that neither quasi-sociometric choices of stimulus persons viewed in a sound movie nor the amount of dominance attributed to these social objects was related to inventory scale scores of dominance for the judges. Jones & Daugherty (117), however, provided evidence that the validity of either similarity or complementarity in social preference depends not only upon the particular personality characteristic examined, but also upon the context and expectations for interaction. They obtained, for example, higher correlations between evaluations of a political stimulus figure and measures of "Machiavellian" tendencies and political values of the judges under conditions of anticipated competitive interaction than under either control conditions or anticipated co-operative interaction. Benedetti & Hill (18) have shown, at least for a condition in which the stimulus person was described as "unsociable," that the sociability of the judges may influence the centrality of that trait in forming impressions of personality (206).

Such accumulating evidence relating a portion of the variance in perceptual and judgmental responses to mediating structures in the judges highlights the importance of treating the area of social perception in terms of consistent individual differences rather than as a kind of psychophysics of social objects where stimulus values are compared in reference to an average judge.

## STYLISTIC STRUCTURES

The present section represents an attempt to bring together under a common rubric several studies emphasizing consistencies in the style of cognition and action, as opposed to content or aptitude consistencies. The distinction between the content of cognitive structures on the one hand and characteristic styles or modes of response on the other has been sharply drawn in several theoretical formulations, and the importance of maintaining this distinction explicitly in the assessment of stylistic and content components in personality measurement has been recently emphasized (115).

*Stylistic variables in perception, judgment, and memory.*—Schachtel (170), noting that the various senses differ not only because they respond to different physical excitations and perceive different attributes but because they represent different types of relatedness between subject and object or different ways of communicating with the world, distinguished between two basic modes of perceptual relatedness—a subject-centered, or “autocentric,” mode with primary emphasis upon affective qualities of sensation and an object-centered, or “allocentric,” mode with emphasis upon objective perception. For Schachtel, the distinction served both to differentiate between so-called lower or autocentric senses, such as taste or smell, and such higher, predominantly allocentric, senses as vision or audition and to characterize a developmental trend in infancy and childhood from a basically autocentric to a primarily allocentric orientation. With an increasing prevalence of allocentric perception, however, a “secondary autocentricity” was considered to develop, whereby certain aspects of objects are emphasized or neglected in reference to the needs, purposes, or fears of the perceiver. Individual differences in the prevalence of autocentric and allocentric modes and differences in the prominence of one or the other in the same person at various times were considered in terms of differences in such general attitudes as intellectualization or pseudo-objectivity, in habitual aspects of personality structure, and in transitory moods.

Gruen (104) described a model specifying four properties of cognitive maps—(a) accuracy of representation, (b) range of outer complexities or the inclusion of stimulus details, (c) range of inner complexities or the inclusion of motivational, affective, and memoric details, and (d) degree of organization or the integration of subparts into patterns—and suggested that individual differences in manifesting these properties represented enduring capacities or characteristic approaches to the environment. Significant relationships were obtained between Rorschach indices of some of these stylistic consistencies and measures derived from a visualization task in which reproductions of learned maze paths were scored for such variables as awareness of stimulus complexities and attempts to organize the maze into patterns of correct moves.

Investigating individual consistencies in attempts to reach perceptual

and cognitive closure quickly, Dittes (64) obtained generally low inter-correlations among several objective measures presumably reflecting reliable tendencies to structure ambiguous stimuli impulsively. A composite index of impulsive closure derived by summing weighted scores on the separate tasks, however, was found to display certain systematic relationships: significantly more closure was shown by subjects under conditions of an experimentally induced threat to self-esteem, for example, and by subjects indicating a greater desire for clarity on some previously administered questionnaires. Dittes considered impulsive closure to be a possible equivalent of such variables as "effort after meaning" (10) or "intolerance of ambiguity" (89). Other related procedures, such as Binder's (26) use of recognition response level to measure tendencies to jump to conclusions on the basis of inadequate cue data, might also be usefully conceptualized in similar terms, provided that controls for specific abilities are introduced to distinguish impulsiveness or "intolerance of ambiguity" as response dispositions from adroitness in correctly satisfying the adaptive requirements of the tasks. Soueif (180) interpreted the response set to use extreme rating categories as a reflection of intolerance of ambiguity and found that scores for this reliable (.92) response tendency were significantly higher for certain social groups presumed to have higher tension levels, such as adolescents vs. adults or Christians (a minority group in Egypt) vs. Moslems. In a factor analysis of inventory scores, Guilford (106) uncovered a dimension whose two definitive loadings were for "black-white thinking" and "need for definiteness," which he used as a basis for conceiving of intolerance of ambiguity as an interest variable. Kenny & Ginsberg (125), however, in a correlational study of 12 questionnaire and objective performance measures of intolerance of ambiguity, found only seven of 66 coefficients (two being in the wrong direction) to be significant at the .05 level, thus emphasizing the specificity of test performance in this area.

Wallach & Kogan (196), in attempting to clarify apparently contradictory evidence concerning sex differences in extremeness and conservatism of judgment, evaluated such differences separately for judgments made with varying degrees of certainty and found that women tended to be consistently more conservative than men when unsure of their decisions and more extreme than men when very sure of their decisions. Frederiksen & Messick (88), in ascertaining the consistency of dispositions toward criticalness in various judgmental tasks, obtained reasonably reliable response-set scores for the tendency (a) to respond "ambiguous" in judging the ambiguity of statements, (b) to respond "different" in appraising the similarity of two possibly alternative expressions, and (c) to respond "different" in deciding whether a revised passage has the same meaning as an original. Since the three criticalness set-scores were found to be substantially intercorrelated, to be generally unrelated either to content measures from the same tasks



or to scores on relevant ability reference tests, and to be altered in the appropriate direction by instructions designed to increase critical attitudes, the response-set measures were hence presumed to reflect a consistent individual style or personality trait, tentatively labeled "criticalness"—an interpretation reinforced by the occurrence of an anticipated significant correlation between uncritical tendencies and acquiescent response set.

Adelson & Redmond (2) interpreted their finding of a significant superiority in verbal recall for "anal retentives" over "anal expulsives," as classified by the Blacky Test, in terms of a cognitive style which is reflected in individual differences in the capacity for attention and concentration during the acquisition process and which is presumably related to differential preferences in the two anal types for such defense mechanisms as isolation as opposed to displacement. Paul (150), in a series of experiments also emphasizing individual differences in remembering, isolated two stable and relatively independent variables called retention ability and importation tendency. The latter was conceived as a stylistic dimension involving a relative reliance at one extreme upon a propensity to import and elaborate themes during the course of serial reproductions and at the other upon a tendency to skeletonize and fragment.

*Field independence.*—In view of its wide range of reported correlates in areas of ability, personality, and social influence, the interpretation of field independence has been gradually extended beyond its original conception, in terms of individual differences in orientation to the upright and in the capacity to differentiate objects from their backgrounds, to represent a more general "active, analytical" orientation as opposed to a "passive, global" one. Witkin (207), in a recent summary of his studies on perceiving the upright, has stressed the development of perceptual style in relation to personality structure and early-life experiences; age curves were presented which indicated that children tend to be field dependent early in their perceptual development and to become more field independent as they grow older, with a slight reversal in the late teens. However, since the children also displayed high stability of performance and were reported to have maintained their relative position within the group over time, a particular child's perceptual style appeared to be established early in life and to remain stable, even though the group averages changed with age. Since maladjusted children were found in both field-dependent and field-independent groups, Witkin also stressed the point that perceptual style by itself does not necessarily imply better adaptation to life situations, but that it may suggest possible relationships with various forms of pathological development or prevalent defenses, as in the finding that male alcoholics tend to be more field dependent than nonalcoholics (208).

Bieri, Bradburn & Galinsky (24) reported significant sex differences in favor of males on Witkin's Embedded Figures Test (EFT), certain Rorschach indices, and a measure of mathematical aptitude. While proficient



EFT performance was consistently correlated with mathematical aptitude in both sexes, it was found to be related to Rorschach M production in one direction for males and in the other for females. Males also exhibited a significant positive correlation between better EFT performance and a preference for complexity in drawings, whereas in females poorer EFT scores were significantly related to a tendency to perceive others in "external" terms. The observed sex differences were accounted for theoretically in terms of two factors—the superior mathematical ability of the males and their more effective combination of this aptitude with a conceptual approach to stimuli. Bieri (23) subsequently conceptualized such sex differences in terms of behavioral variations within each sex on personality variables considered to be closely related to sex role, such as parental identification and acceptance of authority. He found, for example, that females who identify with the father have significantly faster response latencies on EFT than those identifying with the mother and that males who are low in acceptance of authority also show significantly shorter EFT solution times than those high in acceptance of authority; in both cases the results for the other sex and for the two sexes combined were in the predicted direction.

Attempting to confirm and extend some of Witkin's basic findings, Young (210) obtained substantial intercorrelations among scores on the Rod-and-Frame Test (RFT), the Embedded Figures Test, and Barrat's Chair-Window Test, but the expected significant relationships between the factor of field dependence presumably common to these perceptual measures and inkblot scores for coping and introspectiveness were only partially replicated. Meaningful patterns of relationship strikingly consistent with Witkin's conception of field dependence also emerged in an analysis by Crutchfield, Woodworth & Albrecht (58) comparing 10 perceptual measures with over 600 assessment variables on a sample of 100 subjects. As Humphreys (113) has pointed out, however, the likelihood of correlated sampling errors with this research design renders the interpretation of such patterns particularly tenuous.

Gross (103), in appraising the hypothesis that an increase in the uncertainty of perceptual judgments would lead to greater dependence on the environment, experimentally induced a set for uncertainty by giving the impression that a bogus lens used for viewing the RFT apparatus could distort perception, and significantly higher field dependence scores were obtained with the set condition than without it. Linton & Graham (135) presented evidence that subjects susceptible to the influence of the field in perceptual tasks were also susceptible to the influence of persuasion in changing opinions, since opinion changers were generally found to be significantly more field dependent than nonchangers. Wertheim & Mednick (198), noting a similarity in descriptions of the genesis of field independence and of achievement motives, administered both EFT and a

fantasy measure of achievement to the same subjects and obtained a significant correlation. Marlowe (143), on the other hand, found that Thurstone's Concealed Figures Test, used as an alternative to Witkin's EFT, was unrelated to the achievement scale of the Edwards Personal Preference Schedule, as well as to scales for autonomy and dominance (59).

It should be emphasized in attempting to evaluate consistencies and inconsistencies in the above findings that the concept of field dependence was abstracted from communalities in measures of orientation to the upright and of the capacity to differentiate objects from their contexts. Variance specific to single criterion tests, such as RFT or EFT, may possibly confound or even overwhelm expected relationships in any particular application.

*Cognitive controls and cognitive styles.*—Several stylistic variables, including field independence, have been conceptualized by Klein and his co-workers (95, 126, 127) as regulatory tendencies manifested by consistencies in a person's typical modes of perceiving, remembering, and thinking. These cognitive consistencies have been referred to historically under various names such as "perceptual attitudes" and "cognitive system-principles," but the term "control" is currently favored because it clearly reflects the intended emphasis upon regulatory structures (95, 126). Cognitive controls were conceived within the framework of psychoanalytic ego psychology (160) in terms of conflict-free processes which, although closely related to drives as mediating structures, serve adaptive functions in representing and relating to the environment with "individually varying standards of adequacy" (95). Using psychoanalytic theory as a guide, the Klein group has sought generality for cognitive controls in a wide variety of adaptive tasks in an attempt to confirm an hypothesis, also formulated by Thurstone in his broadly empirical foray into the perceptual area, "that the attitudes which the subject adopts spontaneously in making perceptual judgments . . . reflect in some way the parameters that characterize him as a person" (188, p. 6).

In addition to field independence, a number of other cognitive control variables have been thus far delineated:

(a) Leveling-sharpening—a dimension reflecting the degree of differentiation of memory traces and schemata, particularly those representing sequences of stimuli—was investigated in relation to defensive preferences by Holzman & Gardner (112) during the period covered by this review. When Rorschach protocols for 10 extreme levelers and 10 extreme sharpeners selected from a larger group were rated for reliance upon repression as a defense, the six subjects rated high in repression were also found to be extreme levelers, suggesting a possible link between adaptive and defensive processes.

(b) Focusing or scanning, a control principle referring to individual differences in the extent of spontaneous attention deployment, was studied

in relation to Piaget's (156) laws of perceptual activity by Gardner (94), who obtained significant correlations between overestimation in the size-estimation tests long used as criterion measures of focusing and a composite score reflecting overestimation of the standard in two forms of the inverted-T illusion. The finding of negative correlations between size estimation and illusion-effect scores when the "error of the standard" worked against the illusion and of positive correlations when it supplemented the illusion may be taken not only to support Piaget's general centration theory but to illustrate the operation of scanning controls. The finding by Eysenck & Slater (81) of individual differences in the manner in which illusion effects changed as a function of practice and rest may also be relevant in this connection, as may Easterbrook's (66) discussion of "range of cue utilization."

(c) Constricted-flexible control, which refers to consistent modes of reacting to interfering and contradictory cues, has been most frequently assessed in terms of interference scores on the Stroop Color-Word Test, with subjects least susceptible to interference being characterized by flexible control (FC) and those most susceptible by constricted control (CC). Loomis & Moskowitz (138), in attempting to relate measures of constricted-flexible control to differences in the resolution of stimulus ambiguity, provided evidence that FCs were more likely than CCs to integrate contradictory and competing stimulus elements. Hardison & Purcell (108), classifying subjects in terms of both constricted-flexible control and need for independence (using relative scores on EPPS Scales of Autonomy and Deference), noted that when these individual difference variables were ignored the influence of stress upon Block Design Test performance was apparently negligible because independent FCs had shown a significant improvement in performance under stress while dependent CCs had shown a deficit. Broverman & Lazarus (31), incidentally, utilized a different set of scores on the Stroop Color-Word Test (the ratio of speed of naming colors to speed of reading the names of colors) to infer the relative strengths of "conceptual" and "sensorimotor" cognitive subsystems, a stylistic distinction requiring theoretical integration with other cognitive dimensions based upon similar test procedures.

(d) Equivalence range, a control principle reflecting individual preferences in modes of categorizing perceived similarities and differences, is usually assessed in terms of the relative tendency to use many "narrow-range" categories as opposed to few "broad-range" categories in various sorting tasks. Evaluating individual consistencies in the number and width of categories spontaneously employed in judgment tasks, Fillenbaum (86) interpreted his obtained pattern of significant, though moderate, correlations as support for a general tendency toward fineness-coarseness in categorizing behavior. Pettigrew (154), using a task in which subjects were asked to estimate the extremes of a number of diverse categories, obtained

evidence for reliable consistencies in preferred category widths. The tendency to use broad categories was found to exhibit large sex differences and to be significantly related to quantitative aptitude, to Rokeach's "narrow-mindedness" task, and to the number of adjectives checked as self-descriptive, but unrelated to either the F Scale (authoritarianism) or the Dogmatism Scale. Wallach & Caron (194) noted that the tendency to tolerate deviation when judging the similarity of events of varying difference from a standard was correlated with the tendency to use wide categories in Pettigrew's estimation task, a categorizing style discussed here in terms of "conceptual conservatism." Their finding that females generally tolerated less deviation than males was tentatively attributed to a possible "fear of independent expression" in females. Berkowitz (22) also stressed the influence of categorizing tendencies in a theoretical paper attempting to extend the application of judgmental principles from psychophysical to personality functioning.

(e) Tolerance for unrealistic experiences, formerly called tolerance for instability, refers to the differential willingness to accept perceptions at variance with conventional experience; its operation has been noted in terms of Rorschach criteria of form-boundedness and form-lability, recognition time and amount of distortion induced with aniseikonic lenses, and range of apparent movement (95). Eiduson (68), considering the form-lability and form-boundedness aspects of this regulatory principle in terms of flexibility and rigidity, reported a significant correlation between ratings of flexibility in Rorschach performance and in dream imagery, suggesting a consistency of perceptual style at varying levels of awareness.

Since a variety of cognitive controls regulates a person's adaptive behavior, it might be expected that various patterns and combinations of controls would emerge that are characteristic of individuals and that possibly represent "a superordinate level of control within the personality system"; such patterns have been referred to as cognitive styles by Klein and others (95, 126).

Gardner *et al.* (95), in order to appraise possible interrelationships among the above five cognitive controls and field independence, administered measures of all six variables to 30 males and 30 females. Intercorrelation matrices for 33 scores were factor analyzed separately for each sex, and a subsequent analysis producing similar results was performed on 16 variables selected to eliminate overlapping and possibly dependent scores. Two independent factors accounting for only 20 per cent of the total variance were interpreted for the males in terms of scanning and tolerance for unrealistic experiences; whereas three factors accounting for approximately 35 per cent of the total variance were interpreted for the females in terms of field independence, leveling-sharpening, and equivalence range. Although these results might be taken to provide limited support for the independence of five out of the six control principles mentioned, the failure to replicate

factors across sexes raises considerable doubt about generality. This difference in factor structures may reflect a differential reliance between the two sexes upon preferred cognitive controls, but it may also be due to sampling instabilities. Because of the small sample sizes, particularly in relation to the number of variables, the results in general are at best only suggestive, but in the reviewer's opinion, they are suggestive of dimensions of consistent individual differences of such potential importance for a comprehensive treatment of cognition and personality organization that their implications should be considered seriously and further research and refinement of measurement encouraged.

*Response styles.*—The present section deals with stylistic consistencies in response to the form and wording of assessment devices (20, 34, 107), as opposed to consistent responses to item content, with particular reference to the response tendencies to agree and to respond desirably. Major emphasis will be given to articles conceptualizing these response tendencies as stable individual styles or personality traits (115).

Much recent research on the consistent tendency to agree with questionnaire items regardless of their content has centered upon the California F Scale, all the items of which are scored so that agreement supposedly indicates authoritarian attitudes (49, 145). Since the form or tone of item phrasing on the F Scale typically involves extreme wordings and sweeping generalizations, a response set to overgeneralize (115)—a consistent tendency to accept extreme or generalized items as opposed to tentatively qualified ones—may characterize the set component of the F Scale more specifically than an over-all tendency to agree with statements heterogeneous in content and wording. Some evidence for the relative specificity of agreement tendencies as a function of item phrasing has been presented by Hanley (107), whereas the generality of an over-all agreement measure was stressed by Couch & Keniston (57). However, the latter investigators also noted in a factor analysis of their shorter, refined Agreement Response Scale that items with high positive loadings had an "extreme and enthusiastic" tone, while items with high negative loadings were "guarded, qualified, and cautious."

Jackson & Messick (115) emphasized the importance of item form in their demonstration of reliable response-set effects for a scale on which the content of authoritarian items had been counterbalanced but the extreme and generalized style of phrasing systematically maintained. They subsequently recommended the use of four sets of items for appraising various components of F Scale responses—extremely worded positive and reversed items and tentatively worded positive and reversed items.

Although Couch & Keniston subsequently reported that the tentatively worded, reversed F Scale they developed correlated  $-.70$  with an extremely worded, positive F Scale, it would still seem that a balanced score combining these two components could not be interpreted unequivocally in terms

of authoritarian attitudes unless the tendency to agree with extreme vs. tentative phrasings were conceived as, and shown empirically to represent, an integral part of the authoritarian syndrome. This latter contention of a basic psychological similarity between acquiescent response set and authoritarian submission was advanced by Gage & Chatterjee (93) and supported by their finding of higher validities for authoritarian items than for reversed items in predicting principals' ratings of teacher effectiveness in maintaining a nonauthoritarian classroom atmosphere. However, the fact that two variables or response components are both found to correlate with the same criterion does not necessarily imply a statistical relationship between them, especially in view of contrary evidence suggesting the independence of set and content components of authoritarian measures (57, 144).

Partly in apprehension that the label "acquiescence" customarily used to denote the agreeing response tendency had unduly fostered a presumption that "yeasayers" would display acquiescent, dependent, and conforming characteristics, Couch & Keniston (57) developed from 360 items of heterogeneous, counterbalanced content an Over-all Agreement Score (OAS) with which to ascertain the personality correlates of agreement response set and to characterize clinically various personality attributes of extreme scoring yeasayers and naysayers. OAS was found to be internally consistent (.85) and stable over time and to correlate .64 with the tendency to respond "true" on the MMPI and .56 with the tendency to respond "yes" on Cattell's 16 PF Questionnaire.

In discussing their findings, the investigators cautiously affirmed the equivocality of interpreting correlations between OAS and inventory scales with unbalanced keys. However, a similar emphasis might well have tempered inferences drawn from the content of discriminating items or those receiving high loadings on an agreement response factor, especially since their efficacy might have been primarily due to item tone or ambiguous wording. Nevertheless, such an examination of content relationships uncovered consistent patterns of personality correlates for the agreement tendency which were substantially corroborated in subsequent clinical evaluations: Yeasayers were characterized by such terms as stimulus acceptance, extraverted impulsivity, and anal expressiveness, and naysayers by stimulus rejection, intellectual control, and anal suppressiveness. It may be of consequence to note, however, that in this extensive and important investigation of agreeing response set as a personality variable the possible confounding influence of a consistent tendency to respond desirably was not seriously considered or explicitly controlled.

Asch (6), using clinical evaluations of Rorschach, MMPI, and Draw-a-Person performance to distinguish subjects with no response bias from those with an extreme negative bias on inventory scales, found that the general tendency to disagree with neutral, innocuous statements was sig-



nificantly related to ratings of maladjustment and tentatively associated with "neurotic-tendingness" and obsessive-compulsive trends.

Kueth (132) found that positive response set, as measured by the tendency to agree with suggested interpretations in judging the adequacy with which various concepts described inkblots, correlated significantly with performance on a word transcription task when no incentive was offered, but was unrelated to performance under reward conditions. Jackson (114) obtained a significant negative correlation between F-Scale scores and a measure of ability to maintain in perspective a given phase of a reversible cube. He interpreted this relationship in terms of acquiescence and the characteristic energy level with which individuals cope with perceptual field forces, in that subjects acquiescing to F Scale items tended to show little energy in actively coping with the reversing stimulus. Evidence has also been adduced to indicate that verbal and quantitative ability are negatively correlated with acquiescent response set on the F Scale as well as with authoritarian content (32, 50, 144).

The pervasive influence upon scale scores of a general tendency to endorse inventory items considered desirable by social consensus has been widely documented (67). Various measures of this tendency to respond desirably have been shown to be reasonably reliable, highly intercorrelated, and substantially related to a variety of content scales (67, 203, 204), although an exact appraisal of its influence would require the institution of controls for the confounding effects of acquiescence on some of the desirability measures (115, 203). In ascertaining the generality of desirability response set on assessment devices other than inventories, Rozyrko (165) found a significant tendency for subjects scoring high on an inventory desirability measure to produce desirable sentence completions, but Pena (151) obtained a nonsignificant negative correlation between Edwards' Social Desirability Scale and the tendency to attribute desirable content to Rorschach blots.

The validity of rationally constructed desirability scales was assailed by Wiggins (203) who found that when the MMPI was administered under both standard and desirability instructions, certain empirically derived measures of malingering were effective in identifying dissemblers; whereas rational desirability scales turned out to be rather poor predictors of dissimulation. Predictive differences between the two types of measures were attributed primarily to a large acquiescence bias in the rational scales, although it might also be argued that the rational scales had been deliberately designed to measure a tendency to agree with the social norm under standard conditions and that it is immaterial that such scales do not exhibit large mean shifts under faking conditions. In this connection Voas (191) has provided evidence for two relatively independent response tendencies—one a "typical" bias to conform with the group norm and one a "desirability" bias to accept items which, being very desirable but uncommon in the

normative population, exhibit large mean shifts under desirability instructions.

On the other hand, evidence supporting the construct validity of desirability response set as a personality variable has been advanced by Allison & Hunt (4), who noted that subjects scoring high on Edwards' Social Desirability Scale (SDS) expressed significantly less aggression on a paper-and-pencil situational frustration test than subjects scoring low; and by Brown (33), who found, in support of Edwards' (67) hypothesis of a general tendency to "look good" in social situations, that SD scores significantly discriminated good from poor performers on a paired-associates learning task.

The nature of the desirability response set requires considerable clarification with respect to both (a) the possibility of differentiating several specific response tendencies, such as sets to claim undesirable symptoms (61) or to respond in a personally desirable as distinguished from a socially desirable manner (101, 110); and (b) the possibility of resolving within a single framework apparent differences among several rating criteria, such as well-being (62) or adjustment (202), the over-all ratings of which have been found to be very highly correlated with ratings of social desirability despite their probable multidimensionality (85).

#### TEMPERAMENT STRUCTURES

The term temperament is intended here as a broad, somewhat indefinite designation for the consistent affective or emotional propensities and habitual dispositions that typically characterize the manner in which behavior occurs, as distinct from the content of the activity, the ability levels or capacities required for performance, and the motives impelling action. As reflected in the organization of the present review, a distinction is also intended here between the general stylistic dispositions of temperament and the specific cognitive and response styles moderating adaptation. In reviewing research in this area, emphasis was given primarily to multivariate analyses attempting to ascertain or to clarify structural relationships in the general domain of temperament.

In his recent book on *Personality*, Guilford (106) has summarized much of the previous factor-analytic research on dimensions of temperament, giving primary emphasis to orthogonal structures. In an attempt to organize the domain in the fashion of his model for intellect, he has classified established temperament factors in terms of three areas of behavior (general, emotional, and social dispositions) and five kinds of dimensions (positive vs. negative, responsive vs. unresponsive, active vs. passive, controlled vs. uncontrolled, and objective vs. egocentric). Cattell (36), proceeding from the vantage point of oblique simple structure, has also presented a description and integration of factorially established temperament dimensions in the context of the broad, total personality structure that has been

gradually molded out of research findings at his own laboratory over the past 15 years. An excellent summary and brief introduction to Cattell's work has appeared in a well-balanced evaluative review by Sells (175).

Continuing to pursue his extensive research program, with its firm commitment to factor analysis and multivariate experimentation (39, 40), Cattell and his co-workers have reported, during the past two years alone, analyses involving all three of the so-called basic data media—behavior ratings (152, 153) or life criterion observations (L-data), questionnaire responses or Q-data (42, 53, 54), and objective test performances or T-data (43, 44). On the basis of many replicated factor studies interlocked by common marker variables, Cattell (36, 175) has isolated several personality dimensions considered to be reliably established within each data medium: Fifteen dimensions have been extracted from behavior ratings, reflecting such qualities as cyclothymia vs. schizothymia, ego-strength, excitability, dominance, surgency, and superego strength; similar factors with identical names have also been derived from questionnaire data, along with an additional eight dimensions measuring such variables as radicalism, self-sufficiency, and self-sentiment control; and 18 factors have been reported for objective tests, with such labels as harric assertiveness, inhibition, anxiety, and critical practicality. Second-order analyses have been performed separately for L- and Q-data to produce at least four strikingly similar second-order factors in the two media (36, 175): extraversion-introversion, anxiety (45), sensitive subjectivity, and unbroken success. Some of these second-order patterns, particularly anxiety and extraversion for questionnaire data, have been replicated with children by Cattell (38) and with military officers by Karson & Pool (121).

Primarily on the basis of cross-media matching analyses, Cattell has claimed a congruence between most of the behavior-rating factors and their questionnaire counterparts (36, 171) and, at least for anxiety and extraversion, a more tentative linkage between second-order questionnaire factors and objective test dimensions (46). Becker (11), however, has vigorously challenged Cattell's allegation of a one-to-one matching of behavior rating and questionnaire factors and, by a careful perusal of original sources, has documented his counterclaim that reported evidence fails to support Cattell's assertions. Indeed, it would seem that several methodological difficulties, some of which were discussed by Becker, would have to be overcome before substantial cross-media matching could be reasonably expected—in particular, the development of techniques for isolating variance specific to test form and to response biases. In this latter connection it is interesting to note the possibility of response bias effects upon the obtained factor structure in light of the following: (a) Karson's (120) finding that the MMPI K Scale, sometimes interpreted as a measure of both acquiescence and desirability response sets (115), correlated highly with the particular questionnaire factors from Cattell's 16 PF inventory that

define the second-order cluster of anxiety; (b) Bendig's (15) reported correlations between desirability set and the overt and covert components of Cattell's Anxiety Scale; and (c) the fact that marker variables for the objective test factor of anxiety include the tendency to agree and a "willingness to admit common peccadilloes" (36). In reacting to the range and quality of Cattell's total contribution, however, the reviewer found it difficult to phrase an opinion more appropriate or balanced than Sells' (175) impression that in spite of Cattell's sometimes "optimistic and even autistic" interpretations of statistical data, "the grand scope, the systematic classification of tests and factors . . . , the ingenuity of test creation and the insightful and scholarly interpretation and integration of results is a major tour de force."

*Extraversion-introversion and neuroticism.*—Continual research interest has been maintained during recent years in the two basic personality constructs of extraversion-introversion and neuroticism long emphasized by Eysenck. Since several multivariate analyses have presumably established these variables empirically as relatively independent temperament dimensions [e.g., Guilford's R and C factors of rhythymia and emotional instability (106), Cattell's objective test and second-order questionnaire dimensions of extraversion (38) and neuroticism (36, 172), and Eysenck's E and N Scales from the Maudsley Personality Inventory (MPI)] attention has begun to shift toward correlates (13, 14, 116) and theoretical implications (192). In a factor study (82) of questionnaire items both dimensions have recently been replicated. Bendig (16), however, in analyzing relations among inventory scales, found neuroticism to be independent of extraversion but factorially linked to anxiety, suggesting that anxiety and neuroticism are both manifestations of a more general "emotionality" factor. In a factor analysis of 32 MMPI scales, Kassebaum, Couch & Slater (122) obtained two major dimensions which together accounted for 58 per cent of the total variance and which, primarily because of the large number of nonclinical scales used, provided a broader response range than usual. Their factor interpretations were phrased in terms atypical of MMPI studies—ego-weakness (or neuroticism) and extraversion-introversion.

Several studies have appeared during the past two years attempting to evaluate various deductions from Eysenck's (74) inhibition theory, which links extraversion-introversion to characteristic differences in cortical inhibition and, by inference from the similarity of the constructs, to differences in reactive inhibition and neural satiation. Eysenck hypothesized that cortical inhibition is generated more strongly and dissipated more slowly in extraverts than in introverts; hence, extraverts should develop more reactive inhibition and dispel it more slowly than introverts and should be more susceptible than introverts to the development of neural satiation. In evaluating these deductions, however, Rechtschaffen (162) found that visual aftereffects and measures of reactive inhibition from an inverted alphabet-

printing task were unrelated to each other or to extraversion-introversion on Guilford's Rhymia Scale (76). Ray (161), in attempting to extend Eysenck's finding that extraverts show greater reminiscence effects, noted a significantly higher performance trend for introverts during original learning, presumably reflecting the more rapid development of inhibition in extraverts, but found no significant differences in reminiscence between introverted and extraverted groups. Bendig & Vaughan (17), however, found motor learning performance to be unrelated to either extraversion or neuroticism on the MPI.

Eysenck, on the other hand, has reported significant relationships in accord with the theory in studies of problem solving (78) and time estimation (79), and Lynn (141), arguing that academic achievement requires high drive levels and slow accumulation of inhibition, found university students to be significantly higher in neuroticism and lower in extraversion than normative control groups. Eysenck (77) and Bakan (8) also presented evidence consistent with previous findings that introverts condition more quickly and extinguish more slowly than extraverts.

In an attempt to appraise Eysenck's contention that hysterics and psychopaths are extraverted and dysthymics introverted, Sigal, Star & Franks (176) found psychopaths to be significantly more extraverted than dysthymics and dysthymics significantly more introverted than normals. Since neither the hysteric group alone nor the combined hysteric-psychopath sample was significantly different from either dysthymics or normals, however, the authors concluded that either hysterics and dysthymics cannot be used as criterion groups in this context or the E and N scales are not valid, or both. In criticizing certain aspects of the logic and design of this study, which was later defended by the authors (177), Eysenck (75) accepted the finding that normals and hysterics were similar in extraversion (80) but maintained that a revised theory of criterion differences could be readily formulated in terms of psychopathic and dysthymic groups or a theoretical adjustment in the scale placement of normals.

*Graphic movements and temperament factors.*—In an atmosphere of increasing emphasis upon objective performance measures of temperament (36, 142), a renewed interest has been taken in the assessment of possible expressive uniformities in graphic movements. Talmadge (187), in appraising the consistency of several graphic-motor measures on 10 separate reproduction tasks, found (a) an acceptable degree of reliability reflected in an average test-retest coefficient of .62 for 52 scores, (b) substantial individual consistencies in such major variables as personal tempo, pressure, and length of graphic reproductions, (c) consistent relationships between variables, suggesting an underlying structure, but (d) only sporadic and extremely low relationships with the Thurstone Temperament Schedule. However, Wallach & Gahm (195) suggested that graphic movements, although expressive of temperament for nonanxious individuals, would be

compensatory for anxious subjects. Using the E and N scales of the MPI, they found nonanxious extraverts to be most expansive in their graphic movements and nonanxious introverts to be most constricted, but, in line with their hypothesis, anxious introverts were more expansive than anxious extraverts.

#### ATTITUDINAL STRUCTURES

The following important theoretical formulations concerning the structure of attitude, belief, and value systems appeared during the past two years which have broad implications for many other aspects of personality organization: extensive discussions by Newcomb (149) on individual systems of orientation, by Festinger (84) on cognitive dissonance, by Katz & Stotland (123) on attitude structure, and by Rokeach (164) on structural properties of *The Open and Closed Mind*. The considerable literature on authoritarianism, attitude measurement, attitude interrelationships, and opinion change will not be surveyed here, however, to avoid duplication with reviews on social psychology.

#### MOTIVATIONAL STRUCTURES

Since, as Paul (150) has pointed out, many personality "structures are themselves probably crystallizations of recurring motivational states," a section on consistencies in motivation over time and situations seems appropriate in a general review of personality structure. Cattell (36) has attacked the problem of motivational structure by means of several multivariate experimental methods, including such variations as P-technique (205) and incremental R-technique (47). He has recently collated his theoretical formulations in this area and has summarized his experimental findings in terms of rotated factors representing components of attitude strength, "ergs" (innate drives) and "sentiments" (socially acquired drives) (37, 41). Klein (126), Festinger (84), and Kelly (124) have discussed dynamic motivational aspects of cognitive control, cognitive dissonance, and personal constructs, respectively, concepts which also have important structural properties and implications.

A survey of the experimental literature on motivational consistencies, however, revealed a predominance of studies on manifest anxiety and achievement motivation. Although a characterization of individuals as being generally highly anxious or typically motivated to achieve reflects definite structural overtones, such studies would seem to be more appropriately treated in reviews of personality dynamics (7) and will not be discussed here.

#### DEFENSIVE STRUCTURES

Recent theoretical discussions of the mechanisms or enduring psychological structures underlying defensive reactions have emphasized the proc-



esses whereby particular defenses operate, as in Eissler's (69) discourse on isolation, as well as the alternative modes by means of which particular dangers may be defended against. Freud (90), for example, noting that Oedipal threats in adolescence are sensed not only in id impulses and fantasies but in love objects from the Oedipal past, discussed several typical defenses against infantile object ties, such as displacement of libido from the parents to objects outside the family and reversal of affect.

Several recent experimental investigations of defense mechanisms have stressed individual differences in defensive reactions in relation to other personality variables, as in the studies previously mentioned which noted possible connections between preferred defenses and cognitive styles (2, 112). The concept of projection, thoroughly examined recently by Murstein & Pryer (147), was investigated in relation to interpersonal attitudes by Vroom (193), who observed a significant tendency for an individual to project his own attitudes, especially those central to his self concept, to other people whom he liked and to negate the same characteristics in individuals he disliked. Gordon (102) found that "sensitizers," as measured by high admission and low denial on the MMPI Hy and manifest anxiety scales, tended to ascribe similarities to experimental partners significantly less frequently than either "repressors" or neutrals, who did not differ from each other in this regard. This stable response tendency to assume similarity helps clarify the previous finding that sensitizers achieved higher accuracy in predicting responses for people different from themselves while repressors were more accurate in predicting similar partners. Chance (48) reported that sensitizers, selected in terms of high admission and low denial on Welsh's A and R scales of the MMPI, tended to set goals closer to previous performance on level of aspiration tasks than did either repressors or controls, but that the repressors' hypothesized tendency to deny failure by setting high or compensatory goals did not exceed that of the control group. Goldstein (99) found that "copers" and "avoiders," selected in terms of differential sensitivity to the sexual and aggressive implications of various stems in a sentence completion test, were affected differently by fear-arousing propaganda; a minimal fear appeal was slightly more effective than a strong fear appeal in changing responses of avoiders, whereas both types of appeal were relatively ineffective with copers.

The expression and displacement of aggression after frustration, a topic thoroughly reviewed in a general article by Berkowitz (21), was investigated in relation to attitudes toward the self by Worchel (209). He found that although subjects with high self-ideal discrepancies expressed significantly less direct hostility toward the instigator of frustration than subjects with low discrepancies, no evidence for displacement was uncovered.

Sarnoff (166), in evaluating the hypothesis that subjects with high preferences for reaction formation as a defense against affection would become more cynical after experimental arousal of affectionate feelings

than subjects low in reaction formation, found, instead, that both high and low reaction formation groups became less cynical, but that the high group shifted significantly less than the low.

Fulkerson (92) attempted to relate individual differences in reaction to stress to the hysteric vs. psychasthenic typology (72, 186). He observed a slight tendency in a low-adjustment group for high scorers on the MMPI Hy scale to become more inaccurate under failure-induced stress on the McKinney Reporting Test, but no significant trend emerged for the Pt scale in either low- or high-adjustment samples. Caron & Wallach (35) also investigated possible personality determinants of two typical defensive reactions to failure-stress, as reflected by preferences in recall and perceptual recognition for success items (repressive avoidance) or for failure items (obsessive intellectualization). Several measures of variables previously shown to be related to these defenses, such as ego strength, hysteria, and achievement motivation, were factor analyzed to yield five factors interpreted as neuroticism, intellectual flexibility, extraversion, other-orientation, and perseverance for achievement. Correlations between factor scores and recognition and recall measures of defense, however, produced only one significant coefficient out of 15, relating perceptual sensitivity for test-related stimuli to achievement striving.

#### ORGANIZATIONAL CONSTRUCTS

Several articles have appeared during the past two years discussing various ramifications of certain constructs, such as ego and self, which reflect a level of personality organization superordinate to the specific structures summarized in the present review. Ramzy & Wallerstein (158), for example, outlined a structural conceptualization of pain and fear, anxiety (173), and guilt in terms of impingements upon ego boundaries from the outer world, the id, and the superego, respectively. Beres (19) and Spitz (183) conjectured about the development of superego components, and the associated process of identification (118) was widely examined, with particular reference to the development of sexual identity (25, 29, 55, 140). In a recent collection of some of his classic discourses, Erikson (73) discussed his psychosocial theory of ego development and, in particular, the problem of ego identity in adolescence, a problem also investigated empirically by Bronson (28).

*Ego psychology.*—The content domain of ego psychology broadly encompasses the psychological structures mediating adaptation and defense, or, in Rapaport's (159) terms, those "aspects of behavior which are delayable, bring about delay, or are themselves products of delay." As such, many studies relevant to its subject matter have already been reviewed in the previous sections. Psychoanalytic ego theory, which attempts to integrate relationships among these various personality structures and to co-ordinate them with dynamic, economic, and genetic aspects of psychoanalysis, has

been systematically reviewed by Rapaport (160) within the framework of an extensive and detailed analysis of general psychoanalytic theory. However, a few papers have also appeared recently which suggest differential emphases and modifications in basic ego conceptions (119).

Schachtel (170) objected to the postulation of a conflict-free ego sphere (160) to account for autonomous ego functions which did not develop entirely out of conflicts between instincts and reality, and he maintained, instead, that a revision of Freud's concept of pleasure would provide a more adequate basis for comprehending the nature and development of adaptive processes. Thus, in reaction to the orthodox drive-reduction theory of pleasure in psychoanalytic psychology, Schachtel distinguished, in addition to the diffuse tension discharges emphasized by Freud, a positive, directed kind of affect found in encounters "with an expanding reality and in the development, exercise, and realization of growing capacities, skills, and powers." This distinction between the "embeddedness-affect" of tension-release and "activity-affect," with its attendant orientation of "world-openness," provided a context for Schachtel's (170) penetrating discourse on the development of emotion, perception, attention, and memory. Armed with this positive affective basis for adaptive activity, he challenged the position that autonomous functions develop free of conflict and claimed that "every human act bears the trace of the basic conflict of emergence from embeddedness."

White (201) has also recently drawn attention to activity-affect in terms of a "feeling of efficacy" associated with activities such as visual exploration, attention, and thinking, which show "direction, selectivity, and persistence in interacting with the environment." White emphasized the point that activities in the service of effective interaction with the environment, termed "competence," must be considered to be motivated in their own right and to involve satisfaction.

*The self.*—The theory of self, that

organized, consistent conceptual gestalt composed of perceptions of the characteristics of the "I" or "me" and the perceptions of the relationships of the "I" or "me" to others and to various aspects of life, together with the values attached to these perceptions,

has been thoroughly examined in an excellent integrative summary by Rogers (163), in which he utilized the self and related constructs as a basis for theories of personality, of therapy and personality change, of the fully functioning person, and of interpersonal relationships. Spiegel (181) presented a brief clinical survey of disturbances in self concepts and suggested that the self be considered as a frame of reference in internal perception, whereby continuity in time is provided for the orderly cognition of internal states.

Several empirical studies have also appeared during the past two years dealing with self-concept variables (63, 65, 148). Wessman, Ricks & Tyl (200), for example, studied changes in self concepts concomitant with mood fluctuations, and Engel (70), investigating the stability of self concepts in adolescence, found subjects with initially positive self concepts to be significantly more stable than those initially negative. Coopersmith (56) distinguished four types of self-esteem in terms of a combination of subjective inventory responses and ratings of behavior, and Smith (179), in a factor analysis of several bipolar rating scales, uncovered five dimensions of self concept, interpreted as self-esteem, anxiety-tension, independence, estrangement, and body image.

#### OVERVIEW

During the past two years methodological approaches to the investigation of personality structures have included both experimental and correlational techniques, with an increasing trend toward the application of multivariate analysis. In the reviewer's opinion, an acceleration of this trend in investigations at all levels of personality organization would be extremely beneficial in a field with such emphasis upon dimensional variables and structural interrelationships. However, the movement should not be limited to applications of R-technique to a restricted set of measures on a single sample of subjects under specified conditions on a given occasion, but should embrace multivariate experimentation broadly conceived and should utilize such techniques as the variant factor-analytic designs, multiple-discriminant analysis, and multidimensional scaling. The problem of uncovering dimensions of consistent individual differences in learning, judgment, and perception has also recently become focal in psychometric theory, and some current methodological developments show promise for an empirical determination of differential response consistencies in these areas (1, 189, 190).

In attempting to recapitulate research trends in the broad domain of personality structure over the past two years, the reviewer distinctly felt the period to be dominated by an impressive array of intensive integrative summaries of theoretical and empirical endeavors by Cattell (36, 40, 41), Guilford (106), Katz & Stotland (123), Klein and his co-workers (95), Luchins & Luchins (139), Newcomb (149), Rapaport (160), Rogers (163), Rokeach (164), and Schachtel (170).

Another trend in the experimental literature, however, should also be mentioned, although it might not have been reflected in the main review because of the selectivity required. Two single topics, manifest anxiety and response styles, appeared to receive considerable research attention, and, judging from the item properties of anxiety scales and from the tenor of research on response sets, next year a popular research area may be manifest anxiety or response styles.

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## GROUP DYNAMICS<sup>1</sup>

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Group dynamics is a term which has caught the public fancy, and so, in a comparatively short time, has attained a position of prominence in social science lexicon. Although the term is new, the behavior to which it refers is old, and interest in this behavior is perhaps as old as man himself. In many ways, group dynamics is a misnomer. Literally, dynamics refers to the study of the effects of force in producing motion and, thus, implies that group dynamics should deal only with the changing aspects of groups. While it is true that groups are changing continually, it is also true that there are many relatively static aspects of groups that are worthy of study, and are being studied. Perhaps a more descriptive term for the subject matter included in this review would be group processes or simply group behavior.

In preparing a review of this sort, the reviewer must always make certain arbitrary decisions concerning what shall be included and what excluded, how the included material shall be organized, etc. It is traditional for the reviewer to specify at least some of the arbitrary decisions that he has made, and since this writer is basically a conformist, tradition will not be broken. First, although this is the first review devoted exclusively to group dynamics, the general subject matter has usually been included in the review of "Social Psychology and Group Processes"; therefore, the present review is largely, although not exclusively, limited to work appearing within the past year. Second, the review is limited to studies of small, psychological groups. In general, this means that groups larger than about 20 persons are not considered, nor are studies of groups which do not involve interpersonal interaction. For example, investigations which required subjects to react to descriptions or movies of group interaction are not, as a rule, included on the grounds that no direct interaction has occurred.

These decisions were made rather easily, but the final decision concerning organization of material was a real poser. In reading published reports, it soon becomes clear that many researchers are concerned primarily with the phenomenon being studied; e.g., conformity behavior, group development, and the like. Others are concerned primarily with the determinants of the phenomenon; that is, with independent variables. It would be desirable to classify investigations according to either the independent or the dependent variable; however, since studies concerned with a given

<sup>1</sup>The survey of the literature pertaining to this review was concluded in April, 1960.

<sup>2</sup>The author wishes to express his appreciation to Drs. Edgar H. Schein and Wilse B. Webb for reading a draft of this review and offering many helpful suggestions.

phenomenon or determinant tend to be interrelated both in design and in interpretation or theory, it is all but impossible to force phenomenon-oriented studies into a determinant classification or vice versa. Therefore, both classifications are used, and the various studies usually are classified according to the orientations of the investigators.

#### GENERAL AND THEORETICAL WORKS

*Books.*—During the past year a number of major works were published. Bonner (17) undertook the herculean task of organizing and systematizing what is currently known about group dynamics. His *Group Dynamics* is designed as a textbook and attempts broad coverage in contrast to intensive analysis. Bonner concerns himself primarily with group process—with the factors within the group which determine the continual readjustments of group relationships.

The work of Thibaut & Kelley, *The Social Psychology of Groups* (118), contrasts markedly with the Bonner volume. Whereas Bonner attempts extensive coverage, Thibaut & Kelley are analytical in the extreme. Beginning with the dyad as the social unit, they give a detailed analysis of the factors determining the formation, continuance, and nature of interaction. Interaction, it is assumed, is determined chiefly by rewards and costs to the individual members of the dyad. Rewards and costs are, in turn, determined by both endogenous and exogenous factors. Proceeding from these basic assumptions, the authors derive many intriguing hypotheses, with regard to both dyadic interaction and more complex group behavior. It seems certain that this volume will stimulate much research and, thus, will contribute greatly to the understanding of group behavior.

Stogdill's *Individual Behavior and Group Achievement* (111) presents an input-output model that is designed to describe what happens in a group. Inputs are the behaviors of group members, the essential elements of which are performances, interactions, and expectations. These behaviors produce group effects that are exhibited in the form of group achievement, the different aspects of which are productivity, morale, and integration. Unfortunately experimental evidence and theory are discussed separately. This work is directed much more toward larger organizations than that of either Bonner or Thibaut & Kelley.

Although emphasizing leadership, *Leadership, Psychology, and Organizational Behavior* [Bass (9)] is a book largely devoted to group behavior. Bass begins by defining the group as a rewarding collection of people. The more rewarding the group, the more it is effective; the more reward anticipated from membership, the more attractive is the group. The concept of reinforcement thus becomes of paramount importance. Leadership, status effects, and conformity behavior are explained in terms of their relationship to reward potential. Mutual esteem, group size, communications, homogeneity, familiarity, and other group variables are considered to be deter-

minants of interaction potential—the means by which rewards are distributed—influencing group effectiveness. The many propositions concerning group behavior are bolstered by multiple citations from experimental literature. In the reviewer's opinion, the work represents a major contribution to the field; if for no other reason, it will be useful because of the comprehensive bibliography which boasts 1155 entries.

Finally, mention should be made of *The Small Group*, a paperback edition prepared by Olmsted (83). This is an introduction to the study of small groups that attempts to organize and interpret the ideas and research of sociologists and psychologists working in this area.

*Theoretical contributions.*—In addition to the more general theoretical contributions of Thibaut & Kelley, Stogdill, and Bass, mentioned above, a number of theoretical works aimed at more limited aspects of group behavior appeared in the past year. Altman & McGrath (2) published the third of a series of reports concerning an attempt to develop a classification system for the analysis of the literature on small-group research [cf. McGrath (75); McGrath & Altman (76)]. The basis for classification chosen was syntactical rather than substantive and the report has been limited to researches that attempted to measure group productivity. The authors suggested that such a system should result in a detailed statement of existing knowledge, thus providing a basis for more efficient programming of future research. This is an interesting approach and shows promise; however, progress to date has been disappointingly slow since the project still appears to be in the developmental phase.

Harary (46) extended his analysis of group structures by exploring the mathematical properties of signed graphs. Special consideration was given to the assumptions regarding the tendency toward balance, the tendency toward completeness, and the tendency toward positivity in the evolution of human groups. Ross & Harary (97) extended the work on matrix analysis of group structures to a consideration of weakening and strengthening group members defined in terms of connectedness. They reached the interesting conclusion that a group can have no more than two weakening members. No analogous theorems were found for strengthening members, but the authors state that such members can be identified.

The extension of previous approaches to new situations seemed to be the rule during the past year. Suppes & Atkinson (112, 113) followed the rule by extending to multi-person situations their earlier work on the application of statistical learning theory to two-person game situations [see also Atkinson & Suppes (4, 5)]. They had considerable success in predicting group behavior from Markov learning models, but found game theoretic approaches to be unsuccessful in predicting such behavior.

Suppes & Krasne (114) were successful also in applying stimulus sampling theory to the Asch-type conformity experiment. This situation was treated as a stimulus discrimination problem and was analyzed in terms of

statistical learning theory. These applications are further evidence of the increasing sophistication of group process designs and analyses.

The question of deviance behavior was examined by Dentler & Erikson (27). Their analysis is not bolstered by research citations, but some new ideas appear nevertheless. They suggest that deviant behavior tends to be induced by groups and helps maintain group equilibrium; groups thus tend to resist any trend toward alienation of a member whose behavior is deviant. This seems to be contrary to what has been previously observed about group reaction to deviance, but is unusual enough to merit consideration. It may be that group members are ambivalent toward the deviant.

The problem of the optimal distribution of information among group members was considered by Zajonc & Smoke (126). They were able to develop a scheme for distributing a given number of items of information among a given number of individuals in order to obtain maximum probability of each item being recalled by at least one individual. This scheme is independent of amount of material originally given, size of the group, and individual differences in ability. It is limited to aggregates of individuals working independently, but the authors suggest that it can be used to determine a baseline for studying the effects of interaction.

Adams & Romney (1) analyzed authority relations in terms of reinforcement. As they see it, the central idea in authority relations is that of reciprocal control and reinforcement behavior of two persons. Eisman (33) questioned Festinger's nominal definition of cohesiveness. Dupuis (31), somewhat unnecessarily, pointed out that neither the practical, theoretical, nor speculative approach to group dynamics is free from assumptions.

#### GROUP FORMATION AND DEVELOPMENT

An initial question in the study of group behavior would seem to be that of group formation and development. What factors determine whether or not a group will form? What factors contribute to the maintenance of the group after it has formed? Why are new members attracted to the group? Unfortunately, little sound research has been carried out in an attempt to answer these questions. Perhaps, because it is generally assumed that groups satisfy some need or needs in the individual, attempts to deal with need satisfaction in groups are largely limited to investigations of the effects of attraction on group behavior [see Cartwright & Zander (23, pp. 78-80); Davol (26)]. The numerous studies of cohesiveness are examples of this approach.

Current studies have dealt chiefly with the question of what factors influence attraction to the group, rather than simply with the effects of attraction on group process. Aronson & Mills (3) manipulated the severity of initiation to a discussion group and found, as they had predicted from Festinger's theory of dissonance, that severe initiation significantly in-

creased ratings of the value of the group. A study by Dittes (30) also examined factors related to attraction to the group. He showed that persons who were made to feel highly accepted by the group found it more attractive than did persons made to feel poorly accepted by the group, an effect which was greater among persons of low self-esteem. These results were interpreted as illustrating the multiplicative relation between need strength and gratification available in the group as determinants of attraction to the group.

Perceived attractiveness to other persons as a determinant of sociometric choice was investigated by Backman & Secord (7). Each member of 10-person groups composed of like-sexed strangers was led to believe that three others in the group were particularly attracted to him. The designated persons were markedly overchosen as teammates in the first weekly meeting, but this effect did not persist. Actual experience in the group undoubtedly failed to agree with the experimental bias.

Marks (73) hypothesized that interests would be related to group formation. Using social cliques in a high school, he found, with grade and neighborhood level held constant, more variability of interests between cliques than within cliques. In addition, leaders were found to have more extra-clique friendship ties than followers, in agreement with the Whyte-Homans hypothesis.

It is known that environmental factors are related to group formation in housing projects, classrooms, and the like, so it is to be expected that such factors would play a role in other types of situations as well. Sommer (108) examined environmental determinants in the cafeteria of a large mental hospital, using as co-ordinates the positions of the chairs around the tables. His findings are in general agreement with those reported by Festinger, Schacter & Back (38) in that neighbors interacted more than did persons in more distant positions, and those in corners interacted more than others. Sommer also noted that females sat closer to females than to males, and closer than males to either males or females.

Mann & Mann (69) demonstrated that task-oriented study groups resulted in greater behavior and personality change than did discussion groups. Similarly, task-oriented group-interaction resulted in greater change than did self-directed role-playing on intermember ratings of desirability as a friend, leadership initiative, co-operativeness, and general adjustment (70). Although the authors make no attempt to do so, these findings may be related to the theory of group development outlined by Bennis & Shepard (14). It seems to this reviewer that the findings of Mann & Mann are not altogether consistent with the Bennis & Shepard analysis; however, the time span of the present studies was largely limited to the Bennis & Shepard Phase I.

The question of values and acceptance of the group by new members was raised by Hartley (48). By means of a ranking procedure, she ob-



tained estimates of the personal values of new group members, perceived values of the new group, and those perceived as being characteristic of the old group. Value congruity between personal values and perceived values of the new group increased acceptance of that group. Conflict between perceived values of the old and new groups was not related to the new member's acceptance of the new group. In a second study Hartley (49) found that absolute ability of a group to satisfy the needs of its members was more important for the acceptance of that group as a reference group than its comparative standing in relation to other groups in this respect.

Although most of the research effort has been concerned with the factors influencing a member's attraction to the group, the inverse question of factors related to degree of acceptance of new members by the group is of equal importance. Ziller & Behringer (130) turned their attention to this problem. They proposed that a newcomer may be perceived as either a resource person or as an unwelcome source of disruption, depending upon the needs of the group. Assuming that a group with a history of failure would be in need of resources, it was hypothesized that such a group would more readily accept a newcomer than would a group with a history of success. In a situation in which the newcomer had knowledge of the correct answer and a method of arriving at this answer, failure groups were influenced more and rated the newcomer higher than did success groups.

Because there is always danger that behavioral principles will be restricted to the culture in which they are formulated, investigations carried out in other countries are especially welcome. Using the Bales categories, Philp & Dunphy (89) observed developmental changes in problem-solving groups in Sydney, Australia. Their findings, that greatest conflict occurred in the second session, agreed with those in America. The Australian groups differed by showing relatively more task-oriented behavior in the second session, a difference which the authors attributed to greater ego-involvement of the Australian groups because of the greater complexity of the task.

An interesting observational study is reported by Stevenson & Stevenson (110). A racially mixed group of two- and three-year olds was observed in a southern nursery school. Although a majority of the children were aware of race, the physical differences associated with race did not significantly influence either the type or the degree of social interaction among the children.

In summary, the evidence suggests that acceptance by the group, similarity of interests and values, and ecological factors are primary determinants of group formation.

#### PARTICIPATION AND PERFORMANCE

There has been something of a renaissance of research on the effects of participation upon group problem solving, perhaps because of the emphasis

by industrial psychologists upon the desirability of participation. Again, the problem is not new, as attested to by the early studies of group problem solving and studies of group decision. In fact, the old question of individual versus group problem solving may be regarded as a special case of the effects of participation upon performance.

*Individual vs. group performance.*—The current status of this problem is well summarized in a recent review of the literature by Lorge *et al.* (64). They comment that:

In general, in the evaluation of the relative quality of the products produced by groups in contrast to those produced by individuals, the group is superior. The superiority of the group, however, all too frequently, is not as great as would be expected from an interactional theory. In many studies, the product of the "best" individual is superior to that of the "best" group (64, p. 369).

Recent studies by Lorge & Solomon (65) and by Faust (36) give no reason to change this conclusion.

The question of why groups perform better than individuals, when they do, is still a bothersome one. In one of the few attempts to deal with this question directly, Hudgins (55) tested and rejected the hypothesis that groups are more effective than individuals because the group instructs individuals in the proper problem-solving procedure.

*Group decision.*—Enlisting member participation as a technique for reducing resistance to change was first studied by Lewin and his associates (62) during World War II. Since that time an impressive amount of evidence has accumulated to support their findings that greater change is produced when members are permitted to participate in the decision than when they are not. Analysis of the specific factors responsible for this effect, however, has proceeded slowly. This problem was investigated in 1955 by Bennett (13) who concluded that the act of decision making and the degree of actual or perceived consensus in the group, in combination, are sufficient to account for the results of Lewinian-type experiments. Group discussion as such was found to be no more effective than other influence techniques.

This conclusion is questioned by a recent study by Pennington, Haravey, & Bass (86) in which it was demonstrated that opinion change was greater when discussion was permitted than when no discussion occurred. Change also was greater when a decision was made than when it was not, but this variable did not have as great an effect as discussion. One troublesome point in comparing these conflicting results is the definition of group decision. In Bennett's study, as is typical of Lewinian type experiments, group decision refers to individual decisions made in a group setting, whereas Pennington *et al.* deal with group decision defined as consensus. Discussion may be effective when consensus is sought, but ineffective when individual decisions are involved.

That the factors related to the effects of group discussion are many

and varied is further demonstrated by March & Feigenbaum (72). They found that the probability that a group operating under a unanimity rule would adopt the pre-interaction majority judgment of female beauty from photographs depended upon whether that majority favored a critical alternative which was independently identified as exhibiting greater manifest sexuality than other photos considered!

*Brainstorming.*—Group participation may be exploited in a variety of ways and for many different purposes. An example of this is brainstorming, a procedure which has been accepted by many industrial organizations as a means of producing new ideas. Perhaps the most basic rule is that ideas are expressed without regard to quality; no evaluation is permitted, and any idea, no matter how absurd, is acceptable. The whole process is carried out in a group setting in which participation is not only permitted, but expected in the form of "hitch-hiking"—the elaboration of one member's idea by another member. Proponents are convinced that many more useful ideas are elicited by brainstorming than by more traditional techniques.<sup>3</sup>

The faith of industrial brainstormers, apparently, was not enough to convince Meadow, Parnes & Reese (77) who attempted to evaluate the effectiveness of this procedure under controlled conditions. They found that more good solutions, as determined by ratings of a trained rater, were produced under brainstorming instructions than in a control situation. From their report, however, it is not clear whether their subjects worked independently as individuals or in teams.

In an interesting approach to this problem, Taylor, Berry & Block (116) queried whether, when using brainstorming, participation either facilitates or inhibits creative thinking. Individuals who had used the rules of brainstorming in attacking problems were divided randomly into nominal groups of four persons each. Their performance was then compared with that of real four-person groups which had used brainstorming rules in attacking the same problems. The real groups were found to be markedly inferior to the nominal groups with respect to mean number of ideas produced, mean number of unique ideas produced, and mean quality of ideas. Participation appears to be a disadvantage rather than an advantage in this particular type of situation.

The results of the above mentioned studies are interesting and suggestive, but much must be done before anything definitive can be said about the effectiveness of brainstorming.

*Miscellaneous studies.*—A number of other important studies of the effects of participation do not fall readily into any one of the categories discussed above. For example, working with highly participative training groups, Smith & Kight (107) found that personalized feedback from group

<sup>3</sup> An economist once remarked to the reviewer that brainstorming is a kind of poor-man's psychology.

members consistently and significantly improved problem-solving efficiency, and produced some improvement in insight. Contrary to expectations, subgrouping failed to improve either efficiency or insight. Furthermore, group members rated the less effective groups higher on efficiency than the effective ones, a finding which led the authors to suggest that participant ratings may not be an adequate basis for evaluating training procedures. This possibility has long been overlooked and merits serious consideration.

Knutson (60) assigned individuals to groups according to degree of participation in the classroom. Vocal groups, as compared with quiet groups, were happier with their group assignment, expressed greater satisfaction with their production, and were ranked higher by consultants in regard to quality and usefulness of products. These results complement those reported earlier by Riecken (92) which demonstrated that the most talkative member of a group was more successful than the least talkative one in getting a superior solution to a problem accepted by the other group members.

In an interesting study of psychological participation as indicated by questionnaire responses, Vroom (124) corroborated previous findings when participation was shown to have generally positive effects on both attitudes and job performance. It was demonstrated further that the magnitude of these effects is a function of certain personality characteristics of the individual. Equalitarians and those with strong independence needs developed more positive attitudes toward their jobs and increased in performance as a result of participation; authoritarians and those low on need for independence were unaffected by participation.

A problem besetting all students of human behavior is the question of the relative importance of the control attained in the laboratory versus the "reality" attained in natural settings. In an unusual compromise, James (56) studied real jurors in a mock trial situation. Jury proceedings included considerations of personal attitudes and experiences as well as trial procedures and instructions, the latter being emphasized more by the more highly educated persons. Amount of participation by jury members varied in direct proportion to amount of education, but there was no evidence that amount of participation was related to either influence or persuasion.

Simulation represents another attempt to create "real" situations, as illustrated by an investigation by Pepinsky *et al.* (87). In a simulated small industrial plant, team pairs were found to produce more when team expectations of management behavior were confirmed than when their expectations were contradicted by actual management behavior.

Rosenbaum (95) reports that individuals rated partners as being more similar to themselves after discussion under conditions of co-operation than under conditions of competition, and Rosenberg (96) demonstrated that an operant response established in isolation can be maintained when reinforcement is based upon a partner's responses.

## GROUP COMPOSITION

Common sense suggests, and theoretical analyses concur, in positing the characteristics of the individuals who compose the group as one of the chief classes of determinants of group behavior (22, 24). Much research has been devoted to an attempt to support these expectations, but the great bulk of it has been piecemeal, one-shot, unrelated, and generally unorganized. Mann (71) contributed greatly by reviewing the literature dealing with the relationships between personality and performance in small groups and attempting to impose some organization on this heterogeneous mass of data. He uncovered over 500 different personality variables that had been studied in this context, of which 350 could be subsumed under one or another of seven dimensions shown by factor analysis to be important dimensions of personality. These seven factors were then related to six selected measures of behavior. The following relationships appear to be well supported by the data and worthy of repetition here: (a) Intelligence is positively related to total activity rate, leadership, popularity, and proportion of positive social-emotional activity; (b) Adjustment is positively related to leadership, popularity, total activity rate, and positive social-emotional activity; (c) Extraversion is positively related to popularity, total activity rate, and leadership; (d) Dominance is positively related to task contributions initiated and to leadership, and negatively related to conformity; (e) Masculinity bears a low, positive relationship to popularity and leadership; (f) Conservatism is positively related to popularity, task contributions, and conformity, and negatively related to leadership; and (g) Interpersonal sensitivity yields low, positive correlations with leadership and popularity. The many relationships between personality factors and popularity and leadership suggest that these two aspects of group behavior are of a very generalized nature.

Since Mann's review was completed, a number of other studies concerned with group composition have appeared. The results of personality and opinion inventories completed by 819 Air Force majors and lieutenant colonels were factor analyzed by Borg (18). He was able to isolate four factors which he labelled Assertiveness, Power Orientation, Rigidity, and Aggressive Nonconformity. These factors were then correlated with six social roles: popular-social behavior, good follower, assertive behavior, rigid behavior, creative behavior, and leadership. Fourteen of the 24 correlations computed were in the direction hypothesized by Borg, but only Assertiveness appears to have predictive value. This factor correlated negatively with the good-follower role and positively with all the other roles. It is worth noting that, with the exception of the good follower, all roles were positively intercorrelated. It is conceivable that raters simply did not discriminate among roles which specified generally dominant behavior. If so, it is not surprising that Assertiveness correlated rather highly with all of them.

Since the publication of *The Authoritarian Personality*, authoritarianism has been a favorite personality variable. That the composition of the group with respect to this variable influences group behavior in important ways has been demonstrated in a number of experimental settings [see Haythorn *et al.* (52), McCurdy & Eber (74)]. Now Canning & Baker (20) report that the amount of autokinetic movement seen by authoritarians in a group situation is twice as great as in the individual situation, whereas the amount seen by nonauthoritarians in the group is five times as great as in the individual situation.

The study of compatibility of members upon group behavior reported by Schutz (99) has been extended by Sapolsky (98). In one experiment, Sapolsky found that verbal conditioning was greater under instructions designed to create high attraction between subject and experimenter than under instructions designed to create low attraction between subject and experimenter. In a second experiment, Schutz's FIRO-B scales were used to establish compatible and incompatible groups; verbal conditioning was greater in the compatible groups. In both experiments, the effects of conditioning were demonstrated during extinction trials while the experimenter was absent. Sapolsky interpreted this finding as demonstrating suppression of effects in the presence of the experimenter. It is regrettable that suppression effects were not further examined by having the experimenter return to the situation during extinction training.

Four studies are, perhaps, indirectly related to the personality of members and group behavior. Porter & Kaufman (90) reported that scores on a self-description scale correlated significantly and positively with type of verbal interaction and with peer ranking made by group members. Scores correlated positively but unreliably with measures of productivity. Dividing Air Force men assigned to Arctic bases into well adjusted and poorly adjusted groups according to supervisor's ratings, Eilbert & Glaser (32) found the well adjusted, as compared with the poorly adjusted, had fewer interpersonal difficulties, worried less about home events, complained less, scored higher on job proficiency tests, and had a lower sick call rate. Using a similar approach, Mussen & Porter (82) selected highly effective and relatively ineffective members on the basis of sociometric choices. Effective members showed greater *n* Affiliation, greater *n* Achievement, stronger feelings of adequacy, fewer negative self-concepts, and less *n* Aggression than did ineffective members. That inexperienced members tend to react in a manner similar to that of experienced members was demonstrated by Torrance (120) by showing that the acceptability of a new product by group members who had had no experience with the product depended upon whether the reaction of those who had had experience with the product was favorable or unfavorable.

Some attention also has been devoted to sex differences in groups. Vinacke (122) examined sex roles in a three-person competitive game



situation. The two sexes were alike inasmuch as the two weaker members formed alliances against the stronger member if they could win by doing so. They differed in that females more often failed to form alliances, more often formed alliances when none was necessary, more often arrived at triple alliances, and usually agreed upon more proportionate divisions of the prize. Vinacke suggested that males are oriented more toward winning and females more toward working out the most equitable outcome. On the other hand, Handlon & Gross (45) failed to find a sex difference in a situation involving the sharing of an unevenly divisible reward earned by performing a co-operative task.

All in all, there is ample evidence that the characteristics of individuals who compose the group are meaningfully related to group behavior. The great task that remains is the determination of the nature of functional relationships and an analysis of the manner in which the various characteristics interact with each other in the determination of group behavior and process.

#### GROUP STRUCTURES

When individuals interact with each other for any appreciable period of time, differentiation occurs along a variety of dimensions. Some individuals attain higher status than others, some exert more power, some become leaders, and so on. The structure of a group consists of its distinguishable parts and their arrangement with respect to one another [see Cartwright & Zander (23)]. Since the group can be differentiated into distinguishable parts along several different dimensions, the group has not a single structure, but as many different structures as there are dimensions along which it can be differentiated. Research has been directed both toward analysis of the factors influencing the development of group structures and toward the determination of the influence of group structures upon group behavior. An excellent review of techniques used in studying group structures has been prepared by Glanzer & Glaser (43). Sociometric methods, matrix analysis, status indices, and vector analysis are considered in some detail.

*Status and role.*—A number of recent studies have attempted to identify some determinants of status and the effects of status relationships on other aspects of group behavior. Tagiuri, Kogan & Long (115) found that the status of a boy in a secondary school depended upon whether the boy was chosen by others and whether he was seen as choosing the evaluator. Those chosen but not seen as choosing had higher status than those both chosen and seen as choosing—a relationship that was reversed when the status of the chooser reached a very high level. Those perceived as choosing but not chosen had lower status than those chosen. Similarly, Rettig, Jacobson & Pasamanick (91) demonstrated a relationship between the status assigned to a reference group and the status expected from that group.

The accuracy of social perception may be a function of the status of

the perceiver, as shown by the results of a study by Exline (34). His findings suggest, however, that a positive relationship between accuracy of social perception and sociometric status is more likely to occur when group conditions enhance the relevance of accuracy and status to group goals held in common with others. In a related study, Exline & Ziller (35) varied instructions to create the perception that a person was either congruent or incongruent in status with respect to ability to carry out the assigned task successfully and with respect to voting power. As compared with incongruents, congruent groups were more congenial, more often agreed, and were more likely to see others as gaining (and less likely to see them as losing) in ability. Congruents also made fewer errors in reaching a decision as to the number of dots on a briefly exposed stimulus card. Contrary to expectations, congruents did not rate co-workers more favorably nor less critically than did incongruents; neither did disagreements vary with status congruency. This failure was interpreted as indicating the desirability of differentiating the concept of interpersonal conflict into objective and affective components.

The related concept of status consensus, defined as group agreement regarding who contributed most to the attainment of group goals, was investigated by Shelley (104). In girls' clubs, status consensus correlated positively with satisfaction with the group. An individual's own satisfaction also was related to whether the person he ranked first was also ranked highest by the group.

In an interesting study of status and conformity, Harvey & Consalvi (51) demonstrated once more that these two variables are related. Two unusual findings are reported, however. Under financial reward conditions, the second highest status person was found to conform more than either the highest or the lowest status member. Perhaps a more surprising result was the tendency for the effects of the leaders on members' judgments to increase when the latter were rendered in private. These findings merit further study, but a basic question concerning the underlying dynamics of status and conformity is still unanswered. Does the high status person attain status by conforming—or does he conform because he has high status and wants to maintain this position?

As Rodgers (94) points out, the expectations of others regarding the action of the role-taker (role demands) do not always agree with the interpretations of role demands by the role-taker (role concepts). Rodgers was able to demonstrate that role demands of husband and wife place greater emphasis upon spontaneity and independence than do associated role concepts. The further hypothesis that role demands tend to place greater emphasis upon specific acts of behavior and less emphasis upon general personality characteristics than do role concepts was not supported; rather, the results of the study tend to support its inverse. Relationships between certain role characteristics and behavior of workers in a social welfare

agency were examined by Blau (16). Contrary to previous findings in a law enforcement agency (15), number of contacts made by a worker was not related to measures of his expertness. In order to account for this discrepancy, Blau distinguished between social differentiation which is assumed to be hierarchical in nature and social integration which is assumed to be non-hierarchical.

An investigation indirectly related to status and role structure was conducted by Katz & Proctor (58). Changes in sociometric configurations in an eighth grade classroom were shown to conform to a first order Markov chain model. There was no evidence that transition probabilities varied with time, nor were there differences between sexes.

*Communication structure.*—Both the formal and the informal aspects of communication structure have been studied more extensively, possibly, than any other dimension of the group. The informal approach is represented by the many studies from the Research Center for Group Dynamics at the University of Michigan, which have demonstrated that informal patterns of communication are determined by both situational and within-group factors (23). Informal patterns also were shown to be related to such diverse processes as conformity behavior (37), group cohesiveness (38), and rumor transmission (6).

A recent study by Ziller (127) also makes use of the informal approach. He presumed communication systems to be psychologically determined and attempted to measure them by means of F-Scale (authoritarianism) scores, leader conformity scores, and measures of group attraction. Flexibility was determined by the manner in which the group attacked the assigned task which demanded a change in the group's customary operating procedure. Groups whose leaders scored low on the F Scale and moderately high on the conformity scale tended to be more flexible. Greater confidence was expressed by members of high attraction groups and by groups whose leaders tended to conform to the opinions of other group members. Ziller concluded that groups with more open communication systems are more flexible and more confident than those with greater communication restraints.

The study of formal communication structures in small groups is largely limited to the communication network approach suggested by Bavelas (10). Investigations stimulated by this approach have generally been oriented toward one of three aspects. The first group of studies has been concerned primarily with the effects of imposed communication patterns upon group performance and satisfaction. Leavitt's study (61) is the prototype of this class of studies. A second group of investigators primarily treated information distribution in various communication nets and is exemplified by the work of Luce *et al.* (66). Several investigators have been concerned with organizational development in communication nets, an approach represented by Guetzkow & Dill (44).

Studies of communication nets conducted by American investigators have been adequately covered in previous reviews, but a number of inves-

tigations by researchers in other countries have been neglected. Although this reviewer does not pretend to exhaustiveness, some space will be devoted to reviewing these studies as well as the more recent ones carried out in this country.

In France, Flament has examined in detail the question of the relationship between the demands of the task and the communication net. In a series of experiments (39, 40, 41) he has demonstrated that group performance is a function of the degree to which the communication pattern agrees with the communication requirements of the task which the group is asked to solve. Lack of agreement between communication net and task requirements is called "inadequation." The following hypotheses are suggested by Flament's findings: (a) When the task allows for various schedules (operating procedures), the group tends to choose the one that most adequately fits the network; (b) The way in which the performance index is lowered by inadequation depends upon the type of inadequation; (c) Knowledge of the relationships between the schedule and the network improves performance; and (d) Improvement in performance with practice can be explained, in part, by a better knowledge of the relationship between the schedule and the network. Although differing in minor respects, his results are generally in agreement with findings in this country.

In Holland, Mulder (79, 80, 81) has argued that American investigators have placed too much emphasis upon the positional aspect of the group and contended that the attempt to explain satisfaction and performance on the basis of topological variables has failed. He then proceeded to develop and test a number of hypotheses based upon dynamic variables. A methodological variation was the substitution of verbal for written communication. In the first experiment reported, Mulder varied amount of power that could be exerted by a group member, holding amount of activity constant. In agreement with other studies of power differentials (23), he found that power and satisfaction are positively correlated. In a second experiment Mulder verified his prediction that decision structure, defined as the degree to which one person makes the decision for the entire group, would be a determinant of group performance; specifically, he found that the more centralized the decision structure, the more efficiently did the group perform its task. On the basis of these findings, he rejected the so-called participation theory (61, 101), arguing, instead, that saturation of the central person is decreased by a more highly centralized decision structure. Mulder's findings are interesting in their own right, but it is difficult to understand how one can logically reject an hypothesis developed to account for the effects of communication structure by showing that the hypothesis does not account for the effects of decision structure since the two types of structures definitely are not identical.

In a cross-cultural study, Mohanna & Argyle (78) compared centralized (wheel) and decentralized (circle) communication patterns in Oxford and Cairo. Leavitt's symbol identification task was used. Results obtained in

both places were in substantial agreement with results obtained in America—the wheel was more effective with respect to time, messages, and errors. These investigators also demonstrated that an unpopular person in the center of the wheel resulted in this pattern's being less effective than the circle in early trials, but surpassed it on later trials; an unpopular center was less effective than a popular center in the wheel in all trials. The authors suggested that interaction reduced the unpopularity of the central person, resulting in the observed improvement with practice.

The Mohanna & Argyle experiment cited above points to the fact that not only do member characteristics and group structure determine group behavior, but also that these variables interact with each other. The type of structure that will be compatible to a given group may depend upon the kinds of persons who make up the group as suggested by McCurdy & Eber (74). This proposition was tested recently by Shaw (102) in both communication and power structures. In the first experiment it was found that groups led by authoritarians performed relatively better in a centralized communication net, whereas groups led by nonauthoritarians performed relatively better in a decentralized communication net. In the second experiment the performance of authoritarians and nonauthoritarians in a power structure, created by giving one group member complete authority to make decisions, was compared with their performance in an undifferentiated power structure in which all members had an equal voice in making decisions. The results agreed with those of the first experiment in the fact that authoritarians performed relatively better in the power structure whereas nonauthoritarians performed better in the undifferentiated structure, and demonstrated once again that the effects of any given variable are likely to be modified by variations in other factors or conditions.

*Leadership structure.*—The study of leadership may be discussed under a variety of headings, but leadership phenomena seem to this reviewer to be largely structural in nature. When individuals work together in groups, it is a common observation that some persons manifest more leadership behavior than do others and that the differentiated positions are organized in a systematic manner. Leadership studies may inquire about the factors producing these differentiations (structuring), or about the effects of different structures upon group behavior.

By far the greater number of studies have been directed toward identification of the characteristics, personal and situational, that differentiate leaders from nonleaders or that are functionally related to differences in degree of manifest leadership behavior. The past year's research is no exception to this general statement. Kirscht, Lodahl & Haire (59) observed group-chosen leaders in a discussion situation, analyzing the content of the discussion according to Bales' categories. The general picture of the small group leader, as determined from this procedure, shows that he has a high rate of participation, is task oriented, attempts to specify the problem, suggests courses of action, seeks out members' contributions and tries to inte-

grate them, and proposes solutions in an attempt to secure consensus in the group.

Using a somewhat different approach, Bartlett (8) factor analyzed 300 phrases describing leadership behavior in a classroom situation. In addition to a general factor which he attributed to the halo effect, four specific factors were isolated that were interpreted in terms of the ways in which group members can contribute to group discussion: providing ideas and information, creating a friendly atmosphere, working and putting forth effort, and determining policy and making decisions. This study, along with that of Kirscht, Lodahl & Haire, offers further evidence that the leader is typically concerned with the interpersonal and substantive problems of the group.

In a reversal of the usual procedure, Ziller (128) investigated the differences between military leaders who were and those who were not willing to accept responsibility for their group under conditions of uncertainty and risk. Acceptors tended to be relatively unconcerned about differing from group opinion, scored relatively high on the F Scale, and generally were more highly motivated than other leaders.

A study attempting to deal with the effects of leadership structure upon group processes was conducted by Page & McGinnies (84). The same person played the role of directive leader in some groups and that of nondirective leader in others. Group members rated directive leaders more favorably than nondirective leaders, an effect largely accounted for by the ratings of low participants. These findings are difficult to relate to previous findings inasmuch as the dependent variable differed from that measured in earlier investigations. Also, the effectiveness of the leader role depends to some extent upon the particular person playing the role, as shown by Johnson & Smith (57); the leader in the present study might have found the directive role to be more compatible than the nondirective role.

A rather different type of leadership situation was considered by Harvey (50). He identified three types of military leaders: formal leaders who were assigned as squad leaders by military authorities; informal leaders who were selected as leaders by their respective groups; and formal-informal leaders who both occupied the formal leadership positions and were selected by the group. When these leaders were presented with a stimulus differing from that presented to other group members, formal leaders made judgments in accord with the group decision more frequently than informal leaders, whereas the formal-informal were influenced to an intermediate degree. One might speculate that formal leaders feel less secure in their relations to the group than the informal leaders and are attempting to mediate acceptance to the group. This interpretation is consistent with an earlier finding that emergent leaders are more authoritarian than are appointed leaders [Carter *et al.* (21)].

Torrance (121) also attempted to study the effect of leadership behavior upon conformity processes. Members of aircrews were exposed to an in-



structor who introduced a new emergency meat ration. Those who perceived the instructor as exerting no effort to influence reacted most favorably to the new ration. Among those who perceived attempted influence, pressure was effective up to a point, beyond which it tended to operate in a direction opposite to that intended. This suggestion of a U-shaped relationship between pressure and conformity emphasizes the need for exact determination of the relationship between pressure and conformity.

*Power structure.*—Power structure and leadership structure are closely related and often congruent; however, a conceptual distinction can be made because not all powerful persons qualify as leaders, nor do all leaders have power. That power structure can be manipulated in the laboratory is demonstrated by an experiment reported by Chaney & Vinacke (25). They presented convincing evidence to support their contention that coalition formation in triads varies in accordance with initial strength. Coalitions are formed much more frequently between weak members than between either the strong or the weak and strong. Although motivation appeared to have little effect, it was noted that members with strong need to achieve arrived at much better final outcomes than their initial power would indicate.

The relationship between individually prominent behavior and performance was examined in a differentiated and in an undifferentiated power structure by Shaw (103). Scores on a scale to measure tendencies toward individually prominent behavior correlated negatively with measures of group efficiency in the undifferentiated structure, but were unrelated to group effectiveness in the power structure.

#### GROUP TASKS

It appears self-evident that group process should be determined, in part, by the kind of task attempted by the group, a fact recognized by both research and theory. For example, Carter (22) lists the group task as one of the three classes of variables that must be considered in attempting to explain group behavior; representative experimental evidence can be found in the studies by Flament (40) and others (44, 101) cited in connection with studies of communication structures. Despite this general agreement concerning the importance of group task as a variable, relatively little work has been directed specifically toward the determination of the effects of task upon group behavior. During the past year only one study appeared which made explicit use of the task variable. Pepinsky, Pepinsky & Pavlik (88) examined the joint effects of task complexity and time pressure in three-person groups. Groups were found to be more productive when working on a task sufficiently complex to reduce boredom, but the hypothesis that there would be a significant interaction between task complexity and time pressure was not supported by the data. The relationship between time pressure and group productivity was curvilinear regardless of the kind of task.

*Game theory.*—Experimental studies stimulated by game theory may be considered as relevant to this question, although these investigations have

been concerned largely with testing whether human beings actually do behave in the manner predicted by the theory. For example, game theorists make a distinction between zero-sum and nonzero-sum games, the predicted "rational" behavior being different for the two types of situations. While no one has compared the two directly, it appears very likely that the behavior of subjects would be different in the different problem situations. Suppes & Atkinson (112) suggest that nonzero-sum games divide naturally into co-operative and non-co-operative games. These writers have dealt only with the non-co-operative type, but others (100, 125) have considered the co-operative, or bargaining, situation. Schelling (105, 106) has pointed to the fact that in such bargaining games one particular strategy often stands apart from the other possible strategies, thus allowing for intragroup co-operation even in the absence of communication. Since different games have different prominent solutions, the resulting group behavior should be different.

Willis & Joseph (125) put Schelling's ideas to empirical test in a two-person, multitrial situation. Prominence, as defined by Schelling, had little or no predictive value in their experiment, a finding which the authors attributed to imperfect communication and to the fact that the instructions provided incomplete information about the payoff matrix. That the kind of problem does have an effect, however, is demonstrated by their finding that frequency of agreement (the optimal strategy) was strongly and inversely related to the number of alternatives available to subjects on each trial. It is worth noting that frequency of agreement was much less under competitive instructions than under either self-interest or co-operative instructions. It appears that this effect increases with increase in number of categories, although the authors are not clear on this point.

Similarly, Scodel *et al.* (100) tested dyads in a two-choice situation in which monetary rewards were either high or low for both persons if their choices were the same, but high for one and low for the other if their choices were different. Contrary to expectations based upon game-theoretic considerations, players made agreeing choices which resulted in low reward for both more often than agreeing choices which resulted in high reward for both.

Frequency of co-operative choices was also the concern of Loomis (63) who introduced Deutsch's (29) concept of mutual trust as an intervening variable. In the two-person game situation, trust and co-operative behavior were measured as a function of five levels of communication (amount of information given) regarding game relationships. Increase in level of communication increased the probability of establishing perceived mutual trust between members of the dyad; in turn, perceived trust correlated .56 with co-operative choice in the game situation. Communication senders and receivers were approximately equal in trust behavior; however, receivers were responsible for almost 70 per cent of all double-crosses; i.e., making an un-co-operative choice after partner had indicated he would co-operate.

These investigations provide evidence, both direct and indirect, that the nature of the group task exerts significant influence upon group behavior.

#### GROUP SIZE

Group size may be considered as a variable in its own right, or simply as one form of group structure. In this review it is considered separately only because it does not fit nicely into the concept of group structure which was adopted earlier. This in no way denies the importance of size as a determinant of the types of structures which develop in a group.

Group size was given little attention during the past year. On the theoretical side, Parkinson's (85) well-known law has been questioned by Terrien (117). Parkinson suggested that as the organization grows in size, the size of the administrative component increases faster than the size of the total organization because an executive wants to multiply subordinates, not rivals, and officials make work for each other. Terrien has shown that this effect can be due to the internal structure of the organization. Since the percentage of the total number of dyadic relations possible in which a single executive takes part decreases with increased size, the executive must have more assistants in order to maintain control. His suggestion that this can be overcome by decentralization agrees with the results of small-group research as well as the results of research on larger organizations.

A normative study of the effects of group size in welfare agencies was conducted by Thomas (119). In small groups there was greater role consensus, greater breadth of role conception, higher ethical commitment, and a higher quality of work performance. Thomas attributed these results to the fact that small groups were located in smaller communities which made for greater awareness of the needs and demands of society.

A curious size effect was reported by Ziller & Behringer (129). They examined the degree to which an accomplice, who knew the correct answer to a problem and a means of arriving at this answer, could influence the group under conditions of varying group size. Two-person and five-person groups tended to be more accurate, more influenced, and better satisfied than three-person and four-person groups. Do these results mean that there is a curvilinear relationship between group size and influence?

#### INTERPERSONAL PERCEPTION

The obvious and oft-repeated principle that interpersonal relationships begin with the perception of others indicates the importance of this problem area for the understanding of group behavior. A first step in the study of interpersonal perception is the isolation of factors determining such perception; it is not surprising, therefore, that most of the effort to date has been directed toward this end. Recent studies are no exception. Beer *et al.* (11) demonstrated that group members rated leaders significantly higher than nonleaders on confidence, degree of realism, forcefulness, persuasiveness, responsibility, and diplomacy. In self-ratings, leaders showed greater

awareness of how the group feels about them than do nonleaders. By studying supervisor-subordinate pairs, Vroom (123) was able to demonstrate a tendency to project one's own attitudes onto persons whom one likes, and to negate one's own attitudes in persons whom one dislikes. This tendency is greater for characteristics presumed to be more central to the person's self-concept. Similarly, Lundy (68) found an increase in assimilative projection, defined as the degree to which a person perceives another as being like himself, in the second of two periods of interaction, but not in the first. This order effect is a curious one, since an earlier study by Lundy (67) failed to suggest order as an important variable. A further demonstration that the perceiver's own characteristics are important determiners of the perception of others is offered by Benedetti & Hill (12) who found that one's own sociability influences the significance of that trait in the formation of his impressions of others.

An interesting departure from the usual study of perceptions of persons was introduced by DeSoto & Kuethe (28) in their investigation of the subjective probabilities of the existence of various interpersonal relationships. In general, positive affective relationships were regarded by their subjects as most probable, and negative affective relationships as least probable. Furthermore, some relationships (e.g., "likes") were regarded as symmetric and transitive, some (e.g., "confides in") as symmetric but not necessarily transitive, and still others (e.g., "is happier than") as asymmetric and transitive. An essential extension of this research would seem to be a determination of the effects of such expectations upon the types of relationships which actually develop during the course of interaction. Also, it would be interesting to know whether the same kinds of expectations would be obtained in other than college populations.

More central to group behavior, perhaps, is the relationship between the various aspects of person perception and group functions. A start in the right direction has been made by Deutsch (29) by giving attention to the perception of trust and suspicion and the ways in which these perceptions influence interpersonal behavior. The results of several studies reported by Deutsch suggest that there are social situations in which "rational" individual behavior is not possible unless mutual trust is established. In turn, mutual trust is most likely to occur when people are positively oriented toward each other's welfare, although it can occur when people are overtly unconcerned about each other's welfare. Also, the development of mutual trust is facilitated when clear-cut rules exist regarding the consequences of violating trust and when each person has power to influence the other's outcomes.

A highly complicated study of judgmental consistency involving 105 correlations is reported by Borgatta (19). The traits studied were assertiveness, sociability, and emotionality. Measures of these traits were obtained from self-ratings before interaction, and from self-rankings and peer-rankings after discussion of four different topics in three-person and in

five-person groups. Because of an unfortunate communication style, it is difficult to discover exactly what the author's purpose was in carrying out this investigation. The substantive findings indicate that self-ratings are predictors of self-ranking scores that are made after three-person and five-person discussions regardless of the content of the discussions, that peer judgments are to some extent independent of the situations in which they are made, and that self-judgments are positively correlated with peer judgments. If the author draws any conclusion from all this, it is that the consistency of judgments results from stability of qualities and accuracy of perception.

Steiner (109) considered some of the difficulties involved in investigations of the relationship between interpersonal perception and group behavior. This analysis is important because it calls attention to the often overlooked fact that individuals and groups may be quite successful in accomplishing whatever they are trying to accomplish, while at the same time doing rather poorly with regard to the particular performance that the researcher has selected to observe.

#### CURRENT TRENDS

Perhaps the most overwhelming impression one gets from reviewing the literature is that the area of group dynamics is "growing up." Books in the field have advanced from the collection-of-reprints phase (e.g., 23, 47) to attempts to present integrated theories and systematic treatment of substantive content (e.g., 17, 111, 118). The trend toward more sophisticated and more complex research designs in the broader field of social psychology noted in earlier reviews by Gilchrist (42) and by Riecken (93) appears also in the special area of group dynamics. This fact is attested to by the many attempts to develop mathematical models for the description of group behavior, the development of more precise and testable theories, and research directed toward the understanding of more complex group phenomena.

Research in countries other than the United States is rapidly increasing. In a recent review of social psychology in Japan, Hirota (53) pointed out the prominent position of group dynamics in that country. Likewise, psychologists in Australia, France, and Holland, to mention only a few, are becoming more and more involved in the study of group behavior, and collaborative studies by researchers in different countries also are beginning to appear. These contributions are most welcome.

For some reason which is not immediately obvious, there appears to be a trend toward trichotomized theories. Homans' (54) activity, interactions, and sentiments, Schutz's (99) inclusion, control, and affection, and Stogdill's (111) performances, interactions, and expectations are examples of this trend. Can it be there is something magical about the number three?

Some observable trends have both desirable and undesirable aspects. The increasing use of simulation is an example. In the broadest sense of the

term, simulation includes attempts to study group behavior by means of movies, tape recordings, and imaginary group situations, as well as the attempt to develop experimental situations which approximate "real life." In the first type of study, no interaction is engaged in by the persons whose responses are being considered, and it appears doubtful that principles established by these procedures will reflect the consequences of direct interaction. On the other hand, the attempt to devise more and more lifelike experimental situations seems to be a highly desirable advance in group research methodology.

The practical implications of group research are now exerting a greater influence on research undertakings. The *Journal of Applied Psychology* contains an increasing number of articles dealing with group behavior, governmental agencies are supporting "applied" studies of group dynamics, and business organizations are more and more frequently asking about group research. Perhaps this is just another indication of the "growing up" of the field, but there is always danger that application will be made prematurely.

Finally, it is distressing to note a tendency to ignore much of the literature relevant to the problem under consideration. This unfortunate trend appears in both theoretical works and research reports. Authors all too often refer only to studies conducted in their own laboratory, and occasionally to those from one or two other laboratories. This is no doubt due, in part, to space limitations imposed by journal editors and to the increasing difficulty of reviewing all available literature. Whatever the reasons, this is a disturbing trend which one can only hope soon will be reversed.

All in all, the development of the field of group dynamics appears to be healthy. It is still disorganized and a long way from being an integrated body of knowledge, but in a few short years giant steps have been taken. The future looks bright.

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## PSYCHOTHERAPY<sup>1,2</sup>

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The definitive study in psychotherapy was not done this year. It may not be done in any year. But there was research in psychotherapy, and some of it was quite good. This is no longer cause for wonder or self-congratulation. There was a time, not long ago, when research in psychotherapy (any research) was greeted with paternal pats usually accorded to youthful precocity. Now research in therapy is getting on, and it will have to stand on its own merits rather than on its former status as the bright young child of psychology. This it gives every sign of doing.

If this was not the year of the definitive study, it was the year when experimental psychotherapy came into its own. In past years there have been occasional attempts to create laboratory analogs of the therapy situation. In this review they will need a section all to themselves. They may well be increasing by some geometric function. And that is all to the good, certainly, up to a point. More on this later.

### BOOKS AND EDITED WORKS

One of the significant trends in the recent literature of therapy has been the attempt to bring therapeutic theory into the mainstream of general psychology. One evidence of this trend has been the recent efforts to conceptualize therapy in learning-theory terms. These efforts seem to go on at two different levels of discourse. At one level, the phenomena of therapy are translated from psychodynamic terms to learning terms. There is a *post hoc* quality to this level of description—that is, the therapeutic process is reinterpreted or explained in learning terms, but the original therapeutic interaction goes on in the psychodynamic context. An illustration of this level might be the learning reinterpretation of psychoanalytic therapy. Such a reinterpretation can be quite useful as explanation. It may be more limited in its direct feedback to therapy practice.

The second level of discourse is that in which the therapeutic procedure begins from a learning context in the first place, a context in which the therapist acts upon a set of hypotheses cast from the beginning in learning terms. One of the more provocative efforts of this kind during the current year is Pascal's book (80). This treatise is thoroughly imbedded in the behavioristic tradition—so much so that Pascal has no need for clinical terms like patient or therapist, but prefers to think in terms of experi-

<sup>1</sup>The survey of the literature pertaining to this review was concluded in April, 1960.

<sup>2</sup>Abbreviations used in this chapter include: GSR (galvanic skin response); MMPI (Minnesota Multiphasic Inventory); PD (psychological deficit); TAT (Thematic Apperception Test).



menter and subject. This is no mere bit of whimsy on Pascal's part, but reflects, rather, a central premise of the book, namely that the process of behavior change in the clinic is, in principle, precisely like the process of behavior change in the experimental laboratory, or anywhere else for that matter.

Pascal approaches the task of describing behavior change by delineating the concept of psychological deficit (PD). He then sets up an equation which postulates four variables functionally related to psychological deficit. The variables, stated here in simplified form, are stress, habit, psychophylaxis (a resistance-to-deficit factor probably constitutional in nature), and environment. It follows that, if we have an equation in which PD is some function of these factors, PD will be modified by modifying the other terms of the equation. Thus, Pascal's theory of behavior change involves change in any or all of the four variables just named.

Pascal's book is quite slim in size. This is an appropriate reflection of the fact that it is at this point only a preliminary outline of an idea about therapy. A test of the fruitfulness of his ideas may well be reflected in the size of his next revision of the book.

During the current year, several books have appeared that present surveys of varied therapeutic approaches. One of these books, by Standal & Corsini (1965), has an unusual and interesting focal point in its organization. It is built around a series of "critical incidents" in psychotherapy contributed by a number of practicing therapists. Each incident is discussed by several individuals conversant with therapy.

One of the virtues of the book is its close attention to the actual behavior of counselor and client as the incidents and comments are described. Perhaps the chief limitation of the book is a certain fragmentary quality, emphasizing as it does a specific incident in the experience of each therapist. One cannot carry away from it any sustained view of the therapist's day-to-day method. But then this was not the intent of the book.

For some time, teachers of psychotherapy have needed a survey type of book introducing students to diverse viewpoints in psychotherapy. Writers have begun recently to respond to this need. One of the more useful volumes of this kind has been edited by Burton (1964). The book contains 22 case studies illustrating a wide variety of approaches to psychotherapy. Even the geography of the contributions is broad; four of them come from therapists in Europe, and one from Africa.

One cannot cite all of the content, but a few illustrative descriptions may be of value. The chapter having the greatest impact on this reviewer is that by Whitaker, Warkentin & Malone. It illustrates an approach in which the therapists use their own emotional and unconscious processes as the chief way of communicating with the patient. The resulting degree of expressivity makes John Rosen's therapy seem decorous and sedate by comparison.

The authors' verbatim account of the eighth interview is quoted here for illustrative purposes, at the risk of wrenching it out of context. Since this is multiple therapy, the reader will note that two therapists are working with the patient.

*Eighth Interview.* Dr. Brown explained that extraneous circumstances limited the time to twenty minutes today. Early in this interview he had a strong feeling of wanting to feed Hilda, and fantasied himself as having no sexual organs, only breasts; he immediately felt more at ease with the patient and relapsed into silence. Dr. White went to sleep and when he woke up reported a pleasant dream in which he was going down the aisle of the church with Hilda who was dressed in a white bridal costume; they went past the large group of guests and past the chancel; Hilda took off her right arm and laid it on the altar; at this, the groom fainted and Hilda's mother dashed cold water on him, apparently with the intent of killing him; he and Hilda then got out behind the church and mother followed them but never quite got there. To this dream, Hilda made no response as usual, as if she had not heard. (Long silence.) At the end of the interview Hilda went to the door and said her usual "Thank you." By this time Dr. White was so angry that he pushed her with the side of his foot in a rather inadequate, guilty manner. Both therapists felt uneasy about this initial breach of accepted professional conduct.

There is a brief chapter by Munroe, quite nicely written (as always), dealing with the use of intellectualization as a consciously chosen therapeutic tool. Shlien & Lewis present two cases of time-limited client-centered therapy. It is no accident that the cases are reminiscent of Taft's work, particularly in their sensitivity both to time and to emotional communication. Wolpe presents a resume of his work on therapy through reciprocal inhibition. And there are other chapters.

It would be strange if a book like this evoked no reservations. Perhaps, the chief one lies in the nature of the material which the chapters communicate. The book represented a rare opportunity to present a broad range of discussions on therapeutic practice, but the vehicle used, that of case studies, caused some of the contributors to diffuse their efforts in an unsatisfying way. It seems typical for therapists to discuss cases in a manner that fails to differentiate between the psychodynamics of the client and the behavioral interaction of client and therapist. Thus, in some of these cases much time is spent on psychodynamics, and the therapist's behavior is hardly reported. This aspect of the book is not very useful for students, because it conveys a sense of remoteness on the part of the therapist. However, this is hardly a major defect in a book which, on the whole, represents a most welcome addition to the literature on therapy.

Watkins (113) has prepared an outline and annotated bibliography of various approaches to psychotherapy. The magnitude of the literature in the field is well illustrated by the fact that it took a whole book to present a tightly knit outline and reference citations for the area of psychotherapy. The book should prove to be a useful short cut to the student who wants to

note not only the scope of the field but places to find additional information on each of many approaches to psychotherapy.

A brief survey of viewpoints in psychotherapy was published by the American Academy of Psychotherapists, a recently organized but growing interdisciplinary group (30). Papers cover short presentations of seven current approaches to therapy.

A volume that will undoubtedly turn out to be the largest of the year is Volume Two of the *American Handbook of Psychiatry* (3). It must have involved a tremendous amount of organization. Features of the book of special interest in this review include brief descriptions of various approaches to psychotherapy and a more extended account of the variants of psychoanalytic therapy. The chapters together represent a useful, though incomplete, account of current methodologies—incomplete particularly in their omission of the learning-theory and client-centered approaches to psychotherapy. It might reasonably be argued that these are appropriate omissions, since they represent uniquely psychological rather than psychiatric contributions to psychotherapy. But the omissions do indicate some of the nonfunctional boundaries in psychotherapeutic practice. The coverage of the volume is, nevertheless, broad and imaginative. There is a chapter on drug-induced psychoses and several papers on the role of immediate experience in psychiatry, the latter containing a particularly worthwhile introduction to the work of Buber.

Fromm-Reichmann was widely known as a fine person and a sensitive therapist. Bullard (17) has edited a memorial volume in which many of Fromm-Reichmann's papers are brought together. The result is a most worthwhile compendium of her viewpoint on personality and psychotherapy. The papers are characterized by a consistent quality of perceptiveness and warmth. Much of the book concerns therapy with the psychotic person.

Another collection of reprinted articles is offered by Cohen (22). The articles cover a time span of about 20 years and were all originally published in White's journal, *Psychiatry*. About half of the articles are specifically related to psychotherapy and deal with such issues as countertransference, milieu therapy, selective availability of therapy by socio-economic level, and therapy with psychotics.

One of the newly published books describing therapeutic method is that by Moustakas (75). The book deals primarily with concepts of psychotherapy with children, but gives some space also to counseling with parents. An interesting aspect is the wide variety of children considered in the book—e.g., the normal child, the creative child, the disturbed child, the handicapped child, and others. One realizes after finishing that, in spite of these categories, the central premises of therapy are seen as similar for all children. These premises accent the therapist's use of himself in a direct, living personal relationship rather than in a professional-expert role.

Moustakas' book is directly in the relationship therapy and client-centered tradition. It presents themes and concepts familiar to readers of this literature. One may wonder, then, what is new in the book. The answer may be along these lines: client-centered therapy has been in the Rank and Taft tradition, but it developed to a high degree the verbal techniques designed to represent a permissive relationship. The realization came quickly that techniques alone only stand in the way of a genuine relationship, and workers in this sphere have been searching for the way back (or forward) to the relationship itself.

One of the more extensive research reports of the current year comes from Chance's study of families in treatment (20). The central premise of the book is that personality and psychotherapy must be understood in terms of the way an individual experiences interpersonal relationships. Neither the author nor the reader loses sight of this premise at any point in the book because it has shaped the hypotheses, design, and instrumentation of the study. Chance presents a system for classifying interpersonal experiences, and structures the system through a 61-item card sort. She then utilizes this system to describe the therapeutic process as it is seen through the eyes of the children, the parents, and the therapists. Her interest in relationship phenomena leads her not only to assess the perceptions of each participant in therapy, but also to place considerable emphasis on the interrelation of perceptions among the participants. This procedure yields some intriguing findings with regard to the therapists' expectancies concerning their patients. We see that therapists, by and large, develop consistent sets of expectancies which are characteristic for that therapist. These expectancies are not necessarily borne out in the subsequent therapeutic process. Thus, we note that therapists develop norms that are resistant to change through new experience. This may be the price all therapists pay for having a theory.

What about changes in the clients? The most clear-cut finding here relates to the contrast between clients of experienced and inexperienced therapists. On the criterion of self-description, the mothers who worked with experienced therapists showed significant increases in postive-active categories and decreases in positive-passive categories. These changes were not evident for clients of the inexperienced therapists. One might have hoped for similar analyses for the fathers and children.

If one had to assign arbitrary descriptions of this study as a process study or outcome study, the accent on process would clearly win. This emphasis is in harmony with the author's decided wish to stay as close as possible to the clinical data of therapy. This concern is evident at many points throughout the book, along with concern regarding the possibly conflicting goals of research and of therapy. A great many questions about therapeutic outcome are left unsettled. In return, some insights into process are granted.

## THERAPY IN OTHER COUNTRIES AND CULTURES

It has often been said that Americans tend to be provincial in their views and know less about the "outside world" than, let us say, their European colleagues. One American who went out to take a look at how others live was Wright (119). The result is an all-too-brief professional travelogue dealing with rehabilitation practices in European hospitals. In this panoramic view we find striking and dramatic contrasts in concepts of neuropsychiatric care and rehabilitation. On the one hand, Wright describes the radical democracy in Jones's "Therapeutic Community," where patients actually have a voice and vote on whether one of their number is ready for discharge. At the other extreme Wright describes a hospital which seems to be one vast locked ward, where anonymity extended to depriving married women of their rings and married names. It is not easy to suppose that the events described here are contemporaneous with each other.

The close relation between science and ideology in Russia is indicated by the summary of a conference on Freudianism (11). The hero of the conference was Pavlov; the villain, Freud. A number of arguments were advanced, all damning Freudian theory. The quality of the discussion was notably polemic.

In a more objective vein, a Russian study of conditioning with hysteric patients was reported by Korotkin & Suslova (58). Hypnotic suggestion, coupled with age regression, was used. Striking differences in response were noted directly in relation to age regression. The authors suggest that the induced behavior may be further developed by additional training.

Lichko (64) reviews a book published in France on the clinical application of Pavlovian principles. The book begins with a general presentation of Pavlovian theory and then goes on to applications. From the standpoint of therapy, little is claimed beyond induction of relaxation and reduction of pain. The reviewer refers to these procedures as a limited form of rational psychotherapy.

The widespread use of the interview for therapy in our culture may sometimes cause us to confuse form with substance and implicitly to view the interview as a necessary phase of therapy. Studies of other cultures bring refreshing perspective to temper such preconceptions. Opler (78) edited a collection of papers dealing with cross-cultural studies of mental health. Three of the papers deal directly with therapy. Wallace describes cathartic and control strategies used among the Iroquois, and makes a very sensible case for the relation of these therapeutic forms to the organizational level of a society. Dream analysis among the Ute Indians is discussed in a paper by Opler. And Messing deals with group therapy and social status among Ethiopians. There are many other good papers on culture and personality not directly within the purview of this chapter.

Every now and again we are reminded of the continuity of history by the appearance of early and little known works. For those who have as-

sumed that psychotherapy was born with Freud, an interesting historical note by Harms (42) provides other notions. He reports on an 1818 vintage book written by Heinroth in Germany, entitled *Störungen des Seelenlebens* (*Disturbances of the Mind*). The book considers such matters as attitudes of the therapist, indications for activity vs. passivity on the part of the therapist, and other matters which we, no doubt, tend to consider recent inventions. Very salutary.

#### THERAPY AND RELIGION

It has been clear for some time that professional lines of demarcation offer little help in deciding who does psychotherapy. Psychotherapy blends into the functions of many professions. In this connection, one long-term trend has been that the ministerial profession has come to be increasingly sensitive to its therapeutic role. Coincident with this awareness has come the development of clinical training in pastoral counseling. Bruder (16) discusses the present scene and future trends in clinical pastoral training. It is clear that this is a growing trend, and its relative youth carries with it problems and ambiguities with regard to purpose and method. Bruder considers these issues and in doing so introduces some thoughtful clarity to the situation.

In a related vein, Fosdick (34) discusses the emergence of the psychotherapeutic task for the minister. He reminisces concerning the complete absence of this conception during his own pastoral training and the developing awareness of a gap in the minister's function. Fosdick's introduction to the problem came when he announced that he would be available for consultation during a given hour. Fourteen people showed up. It was for such urgencies that pastoral counseling evolved.

Winter (116) speaks of therapeutic counseling as one of the most important aspects of the ministry. He lists and describes four stages of pastoral counseling: facilitation of expression through creative listening; acceptance of the parishioner; helping the parishioner to arrive at an appropriate, reality-oriented relationship to the pastoral counselor; and helping the parishioner in achievement of insight and maturity.

Oates (77) discusses the counseling task of the rural minister and points out ways in which the socio-cultural factors affect the counseling function. His comments about rural psychology are, of course, just as pertinent to the psychologist-counselor as they are to the pastoral counselor. Oates talks about the implications for counseling that result from such factors as the close family solidarity of rural groups, their view of the pastoral function, their position in the class structure, and the minister's isolation from centers of professional resources.

For those of us who are accustomed to considering theology as a relatively static "given," Tillich (110) offers a very provocative corrective. Taking the phenomenon of pastoral psychology as an independent variable,



he deliberates about the ways in which insights from pastoral psychology have influenced theological thought. One influence has been the reformulation of the idea of God. Tillich suggests that psychotherapy has replaced the emphasis on the remote but demanding God with an emphasis on self-giving nearness. In dynamic terms, it modifies the image of the threatening father with elements of an image of an embracing and supporting mother. Such an image of God, Tillich says, emphasizes the concept of acceptance in God's relation to man.

An American Psychological Association symposium took time out to consider some of the relationships between counseling and religion (24, 54, 70, 72, 97). The psychologist is likely to approach this issue with more or less settled personal convictions about the question, and, perhaps, even to consider it a closed issue, something like the church-state question. The symposium members make it clear, however, that the issue not only is still open in their own minds, but that a good many other people continue to think and write on the question. The symposium members make clear some of the very close interactions between values and personality organization. Religious values are reviewed in the context of cognitive processes and ego psychology, concepts of guilt, and ideas about love and acceptance. The issue of "religious views" is thus seen not as a phenomenon grafted on to questions about counseling, but rather as an integral aspect of man's personality and behavior.

#### DRUGS, PHYSIOLOGY, AND THERAPY

In recent years, a number of popular reports have appeared in the press concerning the good effects of tranquilizing drugs. More sober, scientific reports have not always been so glowing. Now, however, we have an objective, large scale study of the relationship between the introduction of tranquilizing drugs and population trends and discharge rate in the New York State mental hospitals (14). It is quite clear that some things did begin to happen after the drugs were introduced. For one thing, the upward trend of admissions was not only halted but reversed. The authors point out that this has not been a transitory phenomenon, but has shown up now for four consecutive years. Another result has been the reduction in use of restraint, seclusion, and psychosurgery. A third result has been the increased number of voluntary admissions. (Does this imply more hope, or more respectability in drug therapy?) And finally, readmissions have been lower for drug patients than for nondrug patients. Naturally, a large scale statistical study of this kind cannot attempt to isolate the factors responsible for the data—e.g., the interaction between drug administration and staff attitudes—but it does point to a relatively stable and socially useful phenomenon.

The hope and enthusiasm that attended the introduction of tranquilizing drugs were watched with mixed response. Some saw in them a revolution in patient care. Others, remembering the history of new procedures, with-

held evaluation. Many of the first experiences, of course, did not involve controls. In a more serious study, Levin (60) compared the effects of several tranquilizers and a placebo upon ward behavior. Results indicated that all groups, including the placebo group, showed beneficial response to the experiment. In a two-month follow-up every group moved toward pretreatment levels. Levin concludes that since improvement during the experimental period could not be attributed to the medication, they were likely to be associated with such factors as suggestion, changed ward routine, and receipt of medication. One sees here another evidence that heightened interaction incident to an experiment may, after all, produce the main effects.

Shapiro (99) puts the issue of the placebo effect into perspective in a delightful and scholarly review of the whole question, starting right from the Egyptians and proceeding through 188 references to the present time. How many other psychologists have forgotten enough of their Latin to have forgotten also that the word placebo means "I shall please?" The definition should make us stop equating the term placebo with inertness or no treatment. The real issue about the placebo is that it is pharmacologically neutral; history records, however, that it is not only a potent relationship element, but, indeed, that through much of medical history it has been a central component of the physician-patient relationship.

Based on the foregoing reasoning, Shapiro concludes his article by suggesting a reverse twist in studying the placebo. Whereas it has typically been used to control or partial out effects, Shapiro suggests a concentration on the study of the placebo effect as the therapeutic agent, with a view toward understanding which aspects of placebo treatment generate real effects. In other words, Shapiro considers that, in view of about 3000 years of experience, we should start thinking about the placebo as an experimental variable as well as a control variable.

Studies in the realm of drug therapy are reported here even though they do not fit, strictly speaking, into the category of psychotherapy. Nevertheless, the implications for psychotherapy are usually clear. A case in point is the study by Watson & Currier (114) on the results of intensive vitamin therapy. The study began with a mean base period of 2.46 months in which each of 30 emotionally disturbed subjects received placebos. The subjects thus served as their own controls for subsequent experimental treatment. No significant changes were noted in Minnesota Multiphasic Inventory (MMPI) scores during the placebo period. During the first experimental period, lasting an average interval of 1.76 months, 22 of the subjects improved ( $p < .01$ ). After the total experimental period, lasting 10 months on average, 24 subjects had improved.

These results constitute a cogent case for vitamin therapy. At the same time, the experimental design raises provocative questions centering around the use of the own-control method. It was evident in the report that a fair amount of personal contact was involved between subject and experimenter,

and that the subject's feelings were the issue of conversation. One might raise here the question of the cumulative effect of such conversation (psychotherapy?) on the subjects' MMPI scores. Anyone raising such an issue would not really have a very strong case, considering the pronounced change in MMPI score after the first short experimental period. Nevertheless, the question sets up a legitimate area of uncertainty. Fortunately, the uncertainty can be dispelled by a single addition to the authors' original design, namely, the institution of a control group which remains on placebos and which has the same amount of contact with the experimenter as that given to the experimental group. The question of cumulative effect of interpersonal contact would thus be resolved.

A study of brief supportive therapy in conjunction with use of tranquilizers was carried out by Cytryn, Gilbert & Eisenberg (25). Three therapy groups were included, two of them being drug groups (strong vs. mild drug) and the third a placebo group. Criterion of improvement was judgment by the two therapists involved as to symptom change and interpersonal adjustment change. Ratings were made without knowledge of the medication received.

In reporting results the research group subdivided the children into four diagnostic categories. Thus, the results could be examined in two ways, by medication and by diagnostic category. Medication yielded no significant effects. However, marked differential outcome was noted for the different diagnostic groups; the degree of difference observed was in the following order, going from largest to smallest change: neurotic, hyperkinetic, defective with behavior disorder, and antisocial.

A further illustration of the combined use of psychotherapy and other procedures can be found in a case report by Bonn & Boorstein (12). They discuss the problems inherent in the nonresponsiveness of some patients to therapy and explore a combination of regressive electroshock therapy and anaclitic psychotherapy. In this procedure the patient is regressed through electroshock treatment. In the ensuing psychotherapy the patient begins at the regressed state and is helped to integrate past experiences as they are remembered. According to the authors, a vital part of the therapy includes giving extra attention to the patient and abundant gratification of the patient's infantile needs.

The majority of studies of adjunctive drug therapy has been done with hospitalized adults. Zier (120) reports a study of the use of meprobamate (Miltown) with 25 "overactive" outpatient children. He reports that of the total sample, 17 were improved in varying degrees. Criterion of improvement consisted of reports by at least two outside sources. In this study, as in others, we need controls to determine concomitants of no-drug therapy.

Millet (74) sketches briefly the history of shock therapies and points out the reservations with which these therapies have been met. From the standpoint of dynamic psychology, the shock therapies have raised issues about

the integrity of the ego. From the religious standpoint the therapies raise questions about doing violence to the domain of the spirit.

Over the past decade, there has been a small but steady incidence of studies concerning physiological aspects of behavior in therapy. Lifshitz & Blair (65) illustrate the potential fruitfulness of this area in a miniature study of physiological processes during hypnotic abreaction of painful memories. They argue quite persuasively that the value of the abreaction phenomenon might be explained in terms of conditioning theory. Specifically, they hypothesize that repetition of a traumatic incident without reinforcement leads to extinction of the affect-laden response. The procedure was to take physiological measures on a single subject during repeated hypnotically-induced response to a traumatic incident. Marked decreases over trials were noted in total response duration, respiratory rate, galvanic skin response (GSR), and muscle activity. In order to control for the possibility that these changes were nonspecific to the particular traumatic incident in question, the abreaction of other incidents was measured at various times. No decreases in physiological activity were observed. Thus, the results were consistent with the authors' hypothesis of an extinction or adaptation phenomenon.

#### DESCRIPTIONS OF METHODS AND PROGRAMS

A method for introducing treatment with unreachable cases was discussed by Slack (100). By unreachable, Slack means the person who refuses to go for therapeutic help. He describes a role relationship derived from experimental psychology, namely, the experimenter-subject relationship (the reader will recall that Pascal also uses this model). Slack points out that a considerable amount of laboratory-type interaction can be therapeutic in nature—e.g., initial warm-up, frequent testing and evaluative procedures, feedback of results, and other kinds of "give and take" which, taken together, serve to develop a relationship. It is important to note that the author is not talking about a ruse by which to snare the person into therapy. He is describing, rather, a way for the experimenter to become known to the person so that a real choice may follow. One interesting sidelight of this method is that, in the neatest switch of the year, the experimenter pays the subject for his services.

Thorne (109) deals with the question of tutorial counseling with mental defectives. He suggests that techniques of possible value would be those which teach the low IQ person to act in any given situation the way a more intelligent person would. Nine principles and techniques are listed, most of them quite naturally being applications of learning theory. Case illustrations are given of the ways these methods may be used.

Ellis (29) reports in some detail his technique of rational therapy by illustrating its use in a single case. Rational psychotherapy is based on the hypothesis that most significant behavior stems from certain basic assump-

tions held by the individual. Neurotic behavior is assumed to be a product of assumptions which are illogical or irrational. The therapeutic technique involves examination of these basic assumptions and instruction in ways of replacing these assumptions with others which are deemed to be more serviceable. The case report turns out here to be a very useful vehicle for illustrating the therapeutic method.

Cowen (23) reports a nationwide survey of institutions having an APA-approved clinical training program with respect to the function of psychologists in personal counseling and psychotherapy services. There was a phenomenal 96 per cent return on the questionnaires. A striking finding was that all 50 of the reporting institutions stated that they provide counseling for undergraduates with personal-emotional problems. Eighteen of the services were located within the psychology department, while 22 colleges maintained separate agencies for psychological and psychiatric services, each carrying on its own function. Only five institutions report facilities which have both psychologists and psychiatrists on the same service staff, though a large majority of the psychological services report the use of psychiatric consultation. Cowen summarizes by pointing out the active role which psychologists are taking in the student mental health programs and the variety of administrative patterns in which the services are organized.

In a description of counseling with graduate students, Kirk (56) identified the frequency and distribution of presenting problems and reported the outcomes as seen by the counselors. One question answered here is whether graduate students utilize counseling services. In this survey it was clear that many did. Primary emphasis was on career planning, confirmation of vocational choices, and problems involved in the transition from school to employment.

#### PROCESS AND OUTCOME: THE THERAPIST, THERAPIST-CLIENT INTERACTION, BEHAVIOR CHANGE, PREDICTION OF CONTINUANCE

Every now and then a heretical proposal is made about the required training for engaging in psychotherapy. Degree requirements have sometimes been questioned, and doubts have been raised about the value of cognitive learning for therapeutic skill. Meanwhile, one occasionally hears reports of an agency or a school system sufficiently unfettered by preconceptions to try out new ways to utilize therapeutic potential. In this vein, Schiffer (96) reports on a training approach in which teachers and counselors are utilized as leaders of therapeutic play groups. Some people question whether it can be done; others go out and do it. Certainly, we have no way of knowing the effectiveness of the training procedures on the therapy being conducted—but that can be said of most training in therapy. The article will be salutary if it does no more than challenge *a priori* assumptions about who should do therapy.

Overlap of function in interdisciplinary therapy settings is widely as-

sumed. In a study of therapy assignments to the different professional groups, Hire & Staver (47) confirmed the supposition that the three therapy professions receive therapy case assignments without systematic rationale for the assignments. It would seem that within the clinic team much of the variance is related to individual competence and reputation of the team member. The study itself, a questionnaire analysis, failed to report frequencies for any category of response and was not as clear-cut as it might have been in its reporting technique.

Another report in the same series (49) considered the training of the team members in the clinical setting. In a study of 142 clinics, it was found that fewer than half of the psychologists doing therapy had Ph.D. degrees. The authors make the interesting point that pressure for raising of professional standards seems to emanate much more from the professional associations than from the clinics themselves.

The question of therapist effectiveness was examined by Knupfer, Jackson & Krieger (57). They were interested in exploring the correspondence between ratings of general competence and specific ratings of therapeutic competence. The authors derived supervisors' ratings of competence in therapy for 40 psychiatric residents and then obtained general competence ratings through a study of the official periodic evaluation reports made out for each resident by the staff. The resulting criterion indices of competence were compared with Q-sorted self-reports by the residents. The emerging picture seems to be of this kind: the high-rated therapists describe themselves in terms connoting self-confidence and expressiveness, whereas the low-rated therapists describe themselves as weaker and more passive. The picture, however, changes to some degree when general competence is the criterion. Here the high-rated residents maintain the self-confidence description, but are more weighted in the direction of control than expressiveness. The authors interpret these findings to indicate, first, that the supervisors' image of a good therapist has some degree of specificity, and, second, that the traits valued for general competence connote "fitting into" an organization—e.g., conscientiousness, control, and compliance.

Streitfeld (107) made a frontal approach to the proposition that better psychotherapists are more accepting than poorer psychotherapists. Using supervisors' ratings, he obtained judgments about therapy outcomes and general therapeutic ability for each therapist. He then administered a scale measuring expressed acceptance of self and others. Analysis of the data did not yield significant correlations between therapeutic ability and acceptance of self or others. One might suspect here that homogeneity of sample precluded high variability on the criterion variable.

Past studies have shown that counselor method is associated more with experience than with stated theoretical orientation. A study by Wrenn (117) moved further into this question. Each of 54 counselors was asked to write responses to standard counseling situations. The situations were ex-



plicitly selected to maximize differences in counselor response in terms of theoretical orientation. The results confirmed the earlier findings that preferred modes of response were independent of professed orientation.

A *post hoc* analysis of data turned up the interesting finding that whereas variability in counselor response was not associated with orientation, it was associated with the counseling situations. A number of significant differences in counselor response were found to be related to elements of the miniature situation. One thus sees procedural differentiation occurring within the counselor with reference to the kind of content with which he deals.

Researchers in psychotherapy have long taken for granted the complexity of the behavior which they are studying. This very familiarity sometimes dulls the edge of perception with regard to this fact. Strupp (108) has tried to step back a bit and recapture perspective on this point. He reminds us that the very fact of continued experience with psychotherapy adds to the complexity of the phenomenon. Thus, although we may have been content earlier to think of therapy in technique terms, the tendency lately is to emphasize the personal qualities brought to the process by the therapist. Such a view suggests that in studying techniques we may not even have been looking at the most pertinent variables in the first place. One answer is the development of progressively closer rapprochement between clinician and researcher. The other is the continued quest for identification and study of the pertinent variables of therapy, complexity and all.

In a presidential address to the APA Division of Counseling Psychology, Berdie (10) analyzes some of the dogmas about counseling which have accumulated over the years. It makes uncomfortable, but instructive, reading to realize how many preconceptions about counseling remain in the literature without benefit of validation.

Shakow (98) lists the chief approaches to the study of psychoanalytic theory and presents a detailed consideration of the filmed psychoanalytic interview as a method of study. He deals with such questions as the privacy of the interviews, subjectivity and incompleteness of the material, the volume of data accumulated, and other matters. He gives fair weight to the problems involved, but it is clear that they are not considered impossible barriers. Shakow then goes on to enumerate briefly the kinds of studies for which recorded material might prove useful.

Malan (68) considers the question of formulating criteria for assessing change in therapy and presents a detailed schedule for content analysis. His outline is based on the premise that an important contribution to the understanding of therapy lies in the understanding of specific changes in psychodynamics as a consequence of therapy. Thus, the outline provides for the enumeration of specific psychodynamic hypotheses for each case.

Meerlo (73) analyzes the phenomenon of free association and considers it from the standpoint of an experiment in communication. He lists

24 categories by which free association patterns may be described (e.g., verbal delay, jocular defense, polarity verbiage). He further lists eight possible bases for silence (e.g., physiologic inhibition, anal erotic silence, aggressive silence).

Pfeffer (81) is concerned with a method for evaluating the results of psychoanalysis. He presents a procedure based on a series of follow-up interviews by an analyst other than the treating analyst. The interviews with the patient are unstructured and similar to the free-association method, and the evaluation of the interview is based on clinical judgment. This technique might well be refined by introducing classification systems around which reliability could be assayed and predictions formulated.

Concepts about termination in psychotherapy have appeared sporadically in the literature for a long time. Freud was interested in the question especially from the standpoint of feasible objectives in therapy. The Rankian school was perhaps most interested in the question. Sullivan seems not to have written on the subject, but recently Chrzanowski (21) has reflected upon the question of termination, using Sullivanian concepts. Essentially, what he does is to set up the touchstones of psychological maturity in Sullivan's terms and examine them as a basis for determining the ending point of therapy.

The validity of self-report inventories has been questioned on various grounds over a period of years. Most often, the grounds for criticism have referred to limitations in honesty and insight. Loevinger & Ossorio (66) examine again the limitations of self-report as indices of therapeutic change. Among the points raised are, first, that the statistically normal or average person is relatively conventional in his thinking, with limited capacity for reflective self-perception; and second, that the average person's antipsychological defenses prevent a candid or insightful self-picture to emerge.

These issues concerning the use of self-report have been persistent ones and continue to need clarification. The authors are surely on firm ground when they object to the self-report as a self-contained validity datum. The problem is that this argument often provides a generally negative halo effect (pitchfork effect?) toward the use of self-reports in general. This would be a mistake. The self-report, like any other class of phenomena, can be most useful if used correctly, rather than in a face-validity sense. The fact is that no single class of data, standing alone, constitutes validity data. The classic requirement for validation involves the necessity for two or more independent estimates of a phenomenon. This requirement holds no less true for self-reports than for any other measure. When used appropriately, then, the self-report may turn up just as useful samples of behavioral regularity or lawfulness as other measures of behavior. It is just that they have no special dispensation from validity requirements. But, then, neither does any other measure of behavior.

The question of values continues to be raised by writers on counseling and psychotherapy. It is a question on which lively differences of opinion may be found. Samler (92) takes a thoroughly unhedged position on the matter and asserts not only that values are involved in counseling, but that they ought to be. Two points are made in this connection. The first is that the selection of values to be implemented is open to study and testability. The second refers to the anchor point for the selection of values—i.e., which values? Here, Samler appeals persuasively to the notion that a very useful place to look for values is in the available models of the mature personality. Thus, the concepts of values and behavioral effectiveness are drawn in converging lines.

Schafer (94) offers an analysis of empathy as it takes place in the treatment situation. He describes what he terms generative-empathy and suggests its component activities. Empathy is seen as a process in which comprehending and experiencing both take place. In this process, though affective components are involved, the ego boundaries of the empathizer remain intact. It is this quality which helps to distinguish empathy from other interpersonal processes. Adequate empathic capacity leads to sensitive understanding and communication of the intricate facets of the patient's experience by the therapist.

Drasgow & Walker (28) propose a method of conceptualizing client-counselor relationships which is organized graphically around relative status. A horizontal relationship is seen as equalitarian, a vertical relationship as superior-subordinate, and a diagonal relationship as having elements of both the horizontal and vertical factors. The graphic concepts are seen as a clarifying force in understanding elements of counseling theory. Further, it is not restricted to the analysis of client-counselor relationships, but to interpersonal relationships in general.

Therapeutic theory has moved more and more in the direction of emphasizing the counselor-client relationship as a central component of therapy. Gendlin, Jenney & Shlien (36) study the concordance between judged success in therapy and a number of relationship factors in the therapy process. They devised a counselor judgment scale containing six relationship items and asked counselors to use the scale after the seventh interview and after the last interview. Three of the six items showed significant correlation with over-all judged success. The successful client is seen as (a) finding the counseling relationship as a particular instance of general relationship problems, (b) deriving new learnings from the counseling relationship, and (c) expressing feelings directly in the relationship (i.e., the experiencing factor).

One of the assumptions concerning competence in therapy is that the therapist understands his patient or client. Harway (44) investigated the extent to which therapists did, indeed, understand their patients. The measure of understanding was the therapists' ability to predict their patients'

scores on the Edwards Personal Preference Scale. Each therapist had had at least 20 interviews with his patient. Utilizing nine therapist-patient pairs, Harway found that eight of the therapists obtained positive correlations. The median correlation of .36 suggests only modest efficiency in the predictions. One sidelight of the study indicated that the therapist's sense of confidence expressed in his predictions turned out to have a significant relationship to his actual knowledge of the patient. It is comforting to note that therapists have a firm perception of reality in this respect.

There are not very many issues in therapeutic theory on which complete agreement is assumed. But we are not likely to get much argument about the assertion that the therapist's personality directly affects the therapist-client interaction. A very nice experimental demonstration of this hypothesis is provided by Bandura, Lipsher & Miller (8). After rating therapists on ease or anxiety in dealing with hostility, the authors studied transcripts of therapy sessions conducted by these therapists. Two points became clear: first, therapists rated less anxious with respect to expression of direct hostility, and lower in need for approval, showed more verbal approach behavior toward patients' hostile expressions; and, second, the approach-avoidance behavior of the therapists had significant consequences for subsequent patient expression—that is, therapist approach responses to patient hostile expression resulted in further expression of this kind by the patient, while avoidance by the therapist led to more avoidance by the patient. One of the pleasant aspects of the study is that the result is compatible with just about any theory of therapy one could name. The authors recognize that studies of this kind could be extended to the effect of attitudes other than hostility on subsequent patient response.

A methodological contribution to the study of therapist-patient relationship was reported by Parloff, Iflund & Goldstein (79). Their work is based on the premise that one factor in therapist-patient communication is their shared understanding of what is important in the therapeutic hour. To arrive at an assessment of this shared understanding, the authors first collected an enumeration of topics which were brought up by the patient during specific interviews. These topics were derived by observing the sessions through a one-way screen. The patient and therapist were then asked independently to rank these topics (a) in the order of importance to them and (b) in the probable order of importance to the other participant in the therapeutic process. In this initial study the sample consisted of one therapist and two patients. Results indicated that the therapist and patient accorded significant similarity to the ranking of the topics, and that each participant could predict to a significant degree the rank order of importance assigned by the other. Interestingly enough, the therapist's predictive ability did not increase after the first hour. The study is not intended as an evaluation of therapeutic change in any sense. The authors have tried, rather, to find some way of assessing a significant dimension of the thera-

peutic interaction. The extent to which therapist and patient understand each other certainly is central enough in almost any theory of therapy to warrant further exploration of methods such as the one offered here.

One of the procedures in psychotherapy which has received considerable theoretical attention is interpretation. All conceivable positions have been taken on the matter, some therapists advocating deep and immediate interpretation, others advising considerable attention to depth and timing, and still others utilizing a minimum of interpretation at any time. A study by Speisman (104) raises the issue of the depth of interpretation in relation to verbal resistance by the client. A seven-step scale of depth of interpretation was devised and applied to interview transcripts. Consequent client responses were analyzed in terms of categories of exploration, on the one hand, and opposition, on the other.

Results indicated that both superficial and moderate interpretation were followed by more exploration and less resistance than that produced by deep interpretations. Moderate interpretation and shifts from superficial to moderate interpretation were followed by a minimum of resistance in relation to the other levels. Speisman concludes that the findings lend support to Fenichel's advice to interpret just beyond the preconscious.

One of the longer term phenomena in therapy research has been continuing efforts by Rogers and his colleagues and students to study the process of therapy. Changing emphases in the studies have been reflections of continued development in therapeutic theory. A study by Walker, Rablen & Rogers (112) reports work on Rogers' most recent formulation of the process of therapy. In this formulation, seven elements of the therapeutic process are identified and scaled on a high-low dimension. The process scale was applied to six transcribed cases which had been ranked according to progress criteria other than the process scale. A comparison of these rankings with rankings yielded by the current process scale showed a correlation of .89 ( $p < .02$ ). One sees a close concordance between the current scale and other indicators of change.

One of the continued issues in studies utilizing this kind of design is the validity implications of process studies. It is suggested here that these studies may be best viewed as conceptualizing studies rather than validity studies. Their contribution lies in the potential usefulness which the classification system has for describing personality and psychotherapy. Taken alone, however, such studies cannot be construed as validity studies. This is so because the essential requirement for validity is that the criterion measure be an independent estimate of the variable under study. A process measure which is compared with rankings derived from the same primary data (i.e., the transcribed interviews) does not meet the test of independence. This argument is not intended to minimize the utility of process studies but rather to focus on their contribution in offering new concepts of therapy which remain to be validated.

Baker (7) deals with the relative effects of two different therapeutic approaches upon discrimination and generalization in reported feelings and behavior. Counselors were trained in two different techniques, one referred to as reflective and the other as leading. Analysis of results disclosed that the two methods were not differentially related to reduction of indiscriminate perceptions or resistance, but that the leading technique reduced personal overgeneralization more than did the reflective technique.

Most studies in psychotherapy may be described as natural experiments in the sense that the counselor response variables are not experimentally manipulated but simply observed. This study is one of the small number in which counselor style is experimentally varied. Counselors were trained to respond in leading or reflective modes. It is a real question as to whether the gain in experimental precision balances the restriction on spontaneous response style induced by the experimental conditions. The point can only be raised here and not answered. But it is one about which many clinicians will think carefully, and about which not a few will shudder.

One of the issues confronting research in therapy is the fact that many different therapeutic approaches seem to yield comparable results. One thus looks for common factors in therapy. Brady, Zeller & Reznikoff (13) inquire into the relation of patient attitudes, and outcome of treatment measures used included a psychiatric-attitudes battery and a degree-of-improvement rating scale. Significant positive relationships were obtained between outcome and the following attitudes: favorableness of conscious attitudes, the patient's view of therapy as neutral rather than pleasurable, and the patient's view of the hospital as protective or supporting rather than threatening.

Bullock & Mudd (18) report on the results of counseling in a sample of 20 cases where in each instance the husband was alcoholic. Evaluation was based on case study by a research group, utilizing records of counseling contacts, statements by the clients, and conferences with counselors. No control group was reported. In eight of the marriages (40 per cent), it was judged that both spouses felt there was improvement. In another two cases, one spouse felt there was improvement. In the other 50 per cent no progress was evident. These results seem consistent with the complexities involved in modifying behavior where alcoholism is an issue.

Graham (40) studied the effect of psychotherapy on frequency and satisfaction in sexual activity. He compared a pretherapy group with groups currently in therapy and found significant increases in reported frequency and satisfaction in sexual activity for the therapy groups.

Studies of a single case sometimes turn up some useful methodological procedures. Brown & Rickard (15) illustrate the use of a particular behavior in evaluating change in therapy. They chose coherence of speech as the variable, and submitted taped excerpts from different phases of therapy to a number of judges. The judges had no difficulty in assigning the ap-



appropriate sequence to the speech excerpts, thus confirming a prediction of increased speech clarity. The issue here is that individual prediction may be a method of choice in evaluation of therapy, based on specific behaviors indicative of disturbance and subsequent progress for that person.

Rioch & Lubin (86) attempted to predict the social adjustment of hospitalized patients under psychotherapy through the use of global Rorschach ratings and the Wechsler-Bellevue. They found a curvilinear relationship indicating that they could say something about patients who would not improve, but not much more. They found that patients with low IQs (i.e., 90 or less) were at the lower end of the adjustment continuum. Similarly, with regard to Rorschach rating, they found that patients with pervasive psychoses did not improve.

One of the difficult problems raised in clinical practice concerns the clients who terminate before they have become really involved in the therapeutic process. Affleck & Mednick (1) ask whether abrupt termination can be predicted. Using the Rorschach as the criterion instrument, they applied the discriminant function and found variables related to significant predictive accuracy. The continuance equation included the response sum (R), movement (M), and human percepts (H). The continuer in therapy is thus seen as a verbally productive person who can respond to human percepts and inner fantasy, and the terminator is seen as lacking in these attributes. The results certainly fit a common sense understanding of what is ordinarily expected of a client in therapy.

A study by Hiler (46) attempted to predict continuation in therapy through the use of a sentence-completion test. He found that out of 100 test items, 15 discriminated significantly between terminators and remainers. Content analysis of the items indicated the following characteristics: remainers were more willing to reveal personal feelings, had more psychological sophistication, had high need for achievement and status, and made frank admissions of inferiority feeling. Hiler cites a 71 per cent prediction accuracy on the cross-validation sample and suggests the utility of the test for selecting therapy patients.

A companion study by Hiler (45) examines the factor of initial complaint as a predictor of continuation in therapy. One of the problems in studying patient variables in continuation concerns the difficulty in extracting the variance due to therapist factors (e.g., age, experience, etc.). Hiler deals with this problem neatly and simply by including in his sample the same number of remainers and terminators for each therapist. Results indicated that remainers had presenting problems more often including obsessions, phobias, depression, poor concentration, and anxiety. The terminators had presenting problems which included acting out, ideas of reference, and other paranoid and schizoid ideation. These results seem congruent with earlier reports that intropunitiveness, anxiety, or both, are related to greater change in therapy than are extropunitive, acting out, or psychotic characteristics.

Whereas the foregoing studies dealt with abrupt terminators, a study by Stieper and Wiener (106) analyzed cases of interminable therapy (range, 2.8 years to 10 years). They compared MMPI changes of these patients with shorter-duration cases and found no differential effects. What they did note, however, was that the majority of long-term cases were accounted for by a minority of therapists. This raised a question as to whether therapist factors rather than patient factors were at issue. Using a pooled judgment method, the authors found a predictable dimension of "dependency nurturing" among the long-term therapists.

In a related study, Wiener (115) raised a question as to the effects of terminating arbitrarily the "interminable" cases. His hypotheses were, first, that no dire consequences would ensue and, second, that returnees could be differentiated from nonreturnees. On the first score, Wiener reports that no case involved a dire consequence resulting from termination. On the second point, he suggests a marginally significant separation between returnees and nonreturnees. The returnees were more likely to have been in treatment longer and be more "psychically" oriented than the nonreturnees.

Studies with adults continue to show selectivity in terms of the kind of person referred for psychotherapy. Bailey, Warshaw & Eichler (6) report a comparison of patients in one center assigned for psychotherapy vs. psychosomatic treatment. The therapy assignees were younger and higher in educational and occupational level. These variables are in accordance with those found in other studies. It is sometimes argued that these data indicate a bias or prejudice on the part of the therapists. While there may be some truth in this, such an argument oversimplifies matters considerably. One table in this study points to another factor, namely, the patient's interest in receiving psychotherapy vs. receiving medication. A markedly larger proportion of the younger and more highly educated patients requested psychotherapy, while a very large majority of patients receiving medication alone requested just that. These facts point to more complex factors implicit in the selection of therapy patients and also to the need for imaginative ways of reaching patients who are not acquainted with the meaning of verbal therapy.

In a companion study using the same sample (5), the authors check the length of stay and amount of progress in therapy in relation to demographic factors. No significant relationships between length of stay in medication treatment and demographic factors were obtained. With regard to psychotherapy, also, most of the variables used did not predict length of stay or progress. The two variables that were predictive were number of years of schooling and previous experience in therapy. The authors conclude that most of the selection criteria do not demonstrate validity.

The literature on social class and continuance in therapy has some provocative ambiguity. It seems to suggest that for adults there is, indeed, a positive relationship between class status and continuation in therapy. For children, however, the evidence is equivocal. Tuckman & Lavell (111)

report the relation between class status and treatment continuance for 11 outpatient clinics. Two facts emerged: first, there was no relationship between social class and continuance in treatment and, second, a disproportionately large number of higher status groups presented themselves for treatment in the first place. These findings continue to keep the issue of social class and psychological services alive and complicated.

One incidental observation noted from the data is that a substantial proportion of clients in all classes terminate treatment before the staff feel that treatment is completed. This has been a fairly typical report from most agencies which have had the fortitude to examine the results of their service, and it raises the general question as to our current state of progress in carrying out the therapeutic process.

A study which examines directly the question of drop-outs is that by Rogers (87). This is a large-scale analysis of data from five departments of Mental Health and one Veterans Administration clinic. The author recognizes that comparisons of these centers involve a good deal of non-systematic variation in age, diagnostic classification, treatment methods, and other factors. The accidental variations inevitably limit the utility of the findings. Major findings were that a high drop-out rate existed and that the mean success rate of patients actually treated was 71 per cent, a figure close to that reported by other studies of success rate in therapy.

#### EXPERIMENTAL PSYCHOTHERAPY

During the past year there has been a continuation of the trend toward laboratory studies in therapy, with gratifying results. There is, for example, the study by Martin, Lundy & Lewin (71) which created three clearly distinguishable therapeutic situations and analyzed the subjects' approach-avoidance verbal behavior in each. The three conditions were those in which (a) the subject spoke into a tape recorder with no one present, (b) the subject met with a therapist who remained silent, and (c) the subject met with a therapist who responded in an essentially client-centered fashion, thus providing a maximum of communicative feedback in contrast to the other two groups. Analysis of both verbal and galvanic skin response (GSR) behavior yielded provocative findings. The major changes noted were changes within interviews, from early to late in any given interview. The most striking differences occurred between the tape group (no therapist) and the regular group (responding therapist). The latter group showed a progressively greater approach tendency as the session proceeded. In GSR response, the regular group showed an initial rise and then a fall in log conductance, whereas the tape group showed a sharp rise as the session proceeded. One, thus, concludes that the regular therapy situation permits progressively greater approach behavior to emotionally toned material with accompanying decrease in reactivity. This is the way matters are supposed to proceed in psychotherapy, and it is very nice, indeed, to find that they do.

It is to be noted that the laboratory studies of quasi-therapy have been moving more and more toward major issues which are pertinent to therapeutic theory. An excellent illustration of this tendency to come to grips with central theoretical issues is the study by Quay (84). His study serves as a timely caution that even those assumptions that have long been taken for granted are going to be re-examined empirically. Quay examined the commonly held assumption that early family memories were just naturally imbedded in a subject's verbal productions. He carried out a study in which family recollections were reinforced for one group and nonfamily memories were reinforced for another group. The expected reinforcement effects appeared in virtually all subjects. One, thus, sees that the frequency of family recollections is a function of therapist behavior as well as subject behavior. It is only a short step further to recognize that therapists may be validating their own theories by selective reinforcement of the desired verbal productions.

There can be no quarrel with selective reinforcement itself. If the therapist believes that exploration of family memories is a helpful procedure, it may well make sense for him to reinforce such memories. The issues, however, are twofold. First, the assumption that these memories are produced spontaneously will need to be revised, and, second, the consequent assumption that continued production of such memories constitutes validation of any theory will need to be re-examined.

Most of the studies in experimental psychotherapy have used simple reinforcements such as "um-hum." These reinforcements have been shown to be more potent in shaping a person's verbal behavior than one might have supposed. Sapolsky (93) carries these studies a step further and introduces the variable of interpersonal attraction or compatibility as a mediator of reinforcement value. He argues, quite logically, that the potency of reinforcement may well depend upon the subject's attitude toward the reinforcer. This is indeed the case. Response acquisitions by the subjects are facilitated by compatibility with the experimenter. Indeed, the findings indicate that incompatibility prevents response acquisition. There was simply no reinforcement effect for incompatible experimenter-subject pairs.

A further illustration of selective reinforcement in laboratory analogues of therapy is to be found in a study by Dinoff *et al.* (27). Here three separate reinforcing areas were set up: environmental responses, self responses, and relationship responses. The experimental task was to tell eight 10-minute stories with a cast of characters to include subject, experimenter, and at least two other people. Reinforcement was given during the middle four stories. Comparison of responses before and after reinforcement indicated significant reinforcement effects.

The relation of anxiety to repression, suppression, and verbalization was studied by Gordon, Martin & Lundy (39). The study was designed to test the hypothesis that there are anxiety gradients which are related to verbalization vs. nonverbalization of conflictual material. Repression and

suppression were defined in terms of posthypnotic suggestion. Anxiety was defined in terms of GSR measures.

The study raises more questions than it answers. One of the major findings, and a straightforward one, was that anxiety increased less during verbalization than during suppression. This is the way therapy is supposed to work. But it is puzzling to find anxiety increasing during repression. The whole idea of repression is that if it works (i.e., if it is complete) it will prevent anxiety. Why, then, the rise in anxiety during repression? The authors invoke the notion that anxiety-producing cues were being produced without awareness. This might very well be so. But we must raise the alternative possibility that repression through hypnosis is just not the same as Freud's kind, and that the subjects were just role playing after all. This idea is not new to the authors. They were the first to mention it. But it points up some of the very difficult problems in inducing experimentally what are basically long-term developmental phenomena.

Kanfer, using a college group (55), studied the relationship between verbal rate and adjustment in relation to specific content categories in a structured interview. Significant correlations were found between verbal rate and adjustment ratings when family relationships and sex were discussed. The relationships were such that high verbal rate coincided with low adjustment or high anxiety in a given content area. The author suggests that the findings are consistent with the Hullian conceptualization of anxiety as a drive state which increases response rate.

A study by Rogers (88) sought to determine the effects of verbal reinforcement on self-references. This class of response was chosen because of its theoretical relevance for psychotherapy. The study indicated that productions of both positive and negative self-references were related to the reinforcing conditions. Thus, it appears that the experimenter can alter the frequency of negative as well as positive responses if he wishes to do so.

Few studies in verbal conditioning have afforded a comparison between normal and clinically disturbed persons with respect to conditioning and extinction rates. Salzinger & Pisoni (91) reinforced affect responses of normal subjects and compared the results with similar procedures carried out earlier with schizophrenic subjects. They found that, whereas rate of acquisition did not differ, the normal subjects showed greater resistance to extinction. In terms of implications for therapy, the authors suggest the possible utility of a larger number of reinforcements for disturbed persons.

In another report on operant conditioning of verbal behavior, Isaacs, Thomas & Goldiamond (51) relate two case illustrations in which verbal behavior was reinstated in psychotic persons after many years of muteness. One should note that these experiments are with actual patients. Using the Skinnerian paradigm of "shaping" behavior, the experimenters continued to reinforce successive approximations to total speech responses

by the patients. In both cases the patients displayed marked increases in verbalization.

Often the therapeutic problem with psychotics is not only to increase normal behavior but to extinguish psychotic behavior. The authors point out that, while it is difficult to extinguish psychotic responses per se, the route to this end lies in the reciprocal relation between normal and psychotic responses—that is, one extinguishes psychotic responses by building up the frequency of normal responses. In this way behavior is “shaped” toward normal response modes.

These studies of experimental psychotherapy show progress both in the complexity of reinforcements employed and in the criterion behaviors studied. They are, thus, moving more and more toward the complex domain of the natural therapy experience. This is all to the good, yet this very fact contains its own risks. For we may be inclined to be lulled into assuming that the results of these studies are directly applicable to therapy itself. We have not yet reached that point, though we may be moving toward it at a pretty fast clip. Yet, there are variables still to be explored. Among them are the length of the relationship, the intensity of interaction, and the motivation for help. Until we have entered these variables into the experiment, it would be premature to equate experimental psychotherapy with the real thing.

#### THERAPY AND REHABILITATION

One of the promising developments in milieu therapy has been the concentration on optimal hospital conditions for effective rehabilitation. In some hospitals, exit units have been established for this purpose. Wright (118) reported a study of the effectiveness of such a program for returning patients to useful community life. The exit unit provided maximum freedom, opportunity for individual and group counseling, and provision for employment experience. Comparisons between exit-unit patients and a control patient group indicated greater gains for the exit-unit group in discharge rate and employment in the community, but not on inventory-type measures. The exit unit thus accomplished the objective of speedier discharge and gainful community employment.

Any study in which an experimental group is visibly differentiated from a control group offers criterion problems which may be inherent in the nature of such an experiment. Ratings by hospital personnel and discharges by physicians take place in a context of known differences between experimental and control groups. How much effect these known differences have on such decisions cannot be known. From the standpoint of practical results, the exit unit comes out clearly ahead. The variables which lead to this difference remain the object of further inquiry.

The foregoing article by Wright mentioned employment as one of the



procedures used for transition from hospital to community. This very useful device has been systematically developed in some Veterans Administration hospitals and given the name "member-employee programs." Gregory & Jacobs (41) advance our understanding of these programs in a descriptive, case-report article. The paper may well represent the conceptual framework from which research studies may be derived.

Another technique of rehabilitation is the trial-visit system, in which hospitalized patients are allowed to visit their homes or communities for limited periods of time. Pishkin & Bradshaw (83) report a study designed to predict response to such trial visits. They analyzed 48 factors and found that eight of them differentiated success and failure. Among the factors which proved significant were age, duration of hospitalization, staff evaluation, group psychotherapy, and source of original referral to the hospital. The use of these factors yielded a better-than-chance over-all prediction of response to trial visits. One notable aspect of recent studies in clinical prediction, including this study, has been the increasing use of the very useful base-rate concept advanced earlier by Meehl & Rosen. They set forth the idea that any study seeking to evaluate the predictive efficiency of a variable must take account of the ordinary frequency of occurrence of the phenomenon to be predicted. In this study the base rate of trial-visit success was 65 per cent. Thus, a significant predictive efficiency requires that the factors under study result in better than base-rate predictive accuracy. Since the study resulted in an 89 per cent accuracy, it was concluded that significant predictive efficiency existed. A natural refinement of this procedure, not yet followed in most of these studies, would be a test of significance of the difference between the base rate and the obtained rate for any given study.

A further transitional device for rehabilitation is the sheltered workshop. These workshops are viewed as a protected environment in which both assessment and training take place, with a view toward greater vocational and personal effectiveness for the individual. DiMichael (26) surveys the uses of workshops, and describes individual and group counseling experiences which have taken place in these settings.

A rehabilitation program for long-stay patients, which combined group therapy with provision for broad patient participation in the communal life of the hospital, is described by Annesley (2). Long-stay patients provided a type of own-control design, since spontaneous recovery is uncommon after this length of stay. One striking aspect of the program was the recognition that a hospital staff often encourages dependence and docility on the part of the patients. Thus, part of the program involved working with staff attitudes. The program resulted in increased discharge rate for male patients, but not for female patients.

Fairweather *et al.* (32) studied the relative effectiveness of four kinds of therapeutic programs. The conditions included (a) work planning only

(the control group); (b) work planning, individual therapy, and exit planning; (c) same as (b), except that therapy was in groups; and (d) group work situation, group living situation, and group therapy. Multiple change criteria were used, including the MMPI, Thematic Apperception Test (TAT), Holland Vocational Preference Inventory, and a follow-up rating scale. In addition, three diagnostic patient categories were studied: nonpsychotic, short-term psychotic, and long-term psychotic. Superiority of the therapy groups was demonstrated. Diagnostic categories discriminated significantly in favor of the nonpsychotic and short-term psychotic groups.

#### GROUP THERAPY

A useful resource for keeping up with the literature on group psychotherapy is the annual review printed in the *International Journal of Group Psychotherapy* (89). Rosenthal & Apaka did the honors this past year. The report is brief but inclusive, covering 112 titles published during the year 1958.

One of the recurrent themes in research discussions has been the conflicting values of the practitioner, on the one hand, and the researcher, on the other. Bennis (9) examines the obstacles to research which stem from this source. As examples, he cites the issue of controls, the question of experimental vs. clinical research, and the "pure" vs. "applied" controversies. He suggests that running through these specific bases are more pervasive attitudinal differences that need to be explored and understood before productive collaboration is possible.

Faure (33) describes a group therapy method which he calls sleep therapy. The elements include a planfully monotonous environment, drug-induced sleep of between 12- and 20-hours-daily duration, and a common room in which meals are taken. The meals are always followed by group therapy. Faure notes that an early stage of therapy is characterized by regression and the arousal of infantile impulses. This stage is followed in turn by stages of aggressive activity and then reharmonization.

An essay on the definition of group therapy is presented by Slavson (101). He distinguishes in detail among the concepts of guidance, counseling, and therapy. The former two are seen as providing immediate, practical, reality-oriented service. Therapy, on the other hand, goes beyond ego-defense processes. This fact, in turn, brings up a whole complex of process events unique to therapy.

The concept of the "therapeutic milieu" flits in and out of the literature, acquiring all manner of connotations and getting a little more elusive each time it appears. Redl (85) objects to such omnibus use of the term and specifies an even dozen useful attributes of a therapeutic milieu. His article, besides being delightfully written, offers clarity to the use of the term.

Jones (52) discusses work therapy as a form of group therapy, and illustrates his point by describing some of the therapeutic methods used

with five work groups at Belmont Hospital in London. The program involves many staff members ordinarily regarded as nonprofessional people, and considerable emphasis is placed on their inservice training in psychological sensitivity.

The interrelationships between group dynamics and group psychotherapy are explored in two thoughtful articles by Scheidlinger (95). His particular interest is in arriving at some meeting ground for understanding these two phenomena within the context of psychoanalytic theory. One of the problems in this connection has been the paucity of theorizing about groups within the classical psychoanalytic framework. Scheidlinger turns to the work of Bion and analyzes in detail some of Bion's views in relation to Freudian theory and group psychology.

There are frequent references in the literature to combined individual and group psychotherapy. Sager (90) discusses the compatibility of group therapy and individual psychoanalysis with the same therapist. He sees the combined treatment as entirely feasible under certain circumstances. Two patient criteria, in particular, are the prior formation of transference in individual analysis and a sufficient degree of ego strength to withstand attacks by other group members both on the patient and his analyst.

A therapeutic program for gifted children, carried on in an educational setting, is described by McIntyre (67). Children with IQs of 148 or above, who could not adjust in the regular classroom, were assigned to an adjustment class taught by specially selected teachers. The more disturbed pupils were referred for help to the Pupil Study Center, a clinic within the Guidance Department. Close co-ordination between the Study Center and the classroom is maintained.

Azima & Azima (4) describe a technique which they call projective group therapy. The chief emphasis of the method lies in the use of the objects created by the patients as externalized symbols of ego and need systems. The therapy hours are divided between a free creation period in which the objects are made and a free association period in which the patients discuss the created objects.

Extension of therapeutic principles to classroom groups frequently centers around concepts of permissiveness and sensitivity to feelings. Harris & Sievers (43) report a study of the effects of permissiveness on behavior in a group of mentally retarded girls. A number of content dimensions were devised and ordered on a positive-negative dichotomy (e.g., acceptance of peers, rejection of peers). The teacher recorded the behavior at the time it occurred. Analysis of data indicated that positive behavior increased significantly from the first to the second six-month period. Thereafter, the positive behavior was maintained but did not increase further. With regard to negative behavior, more variability and less clear-cut trends emerged, with results favoring the hypothesis that negative behavior decreased particularly during the middle time spans of the two-year study.

One of the best controlled studies in the literature during the current year is the study by Snyder & Sechrest (103), who set out to evaluate the effect of group therapy with mentally defective delinquent boys. In their review of the literature, the authors point out the minimal results obtained with such groups in the past and conclude that a directive and highly structured therapeutic environment would offer the greatest chance of success. The design included three equivalent groups: a therapy group, a control group, and a placebo-group which met for unstructured discussion. The placebo-group notion is certainly an intriguing one; this reviewer, however, found it difficult to discern just what was being experienced or controlled by that group, and the authors express some uncertainty on this same point. Nevertheless, the idea ought to be pursued further and clarified as to structure and intent.

The criteria of change included reports of rule violations and "house reports," both positive and negative, made out by ward officers. Results of the 13-week experiment showed that the therapy group received more positive ward-officer reports than the other groups, and fewer reports of behavioral infraction. Thus, one can assume a real effect with respect to the question of behavioral control. One would like ultimately to know more than this, particularly with regard to questions of personality reorganization. But no one study will do all this for us, and the careful design illustrated here will eventually move us ahead in this direction. It is a striking rarity in the literature of therapy to find groups equivalent (randomized) with regard to motivation for therapy and personality factors.

There is no doubt a moral here, with regard to the kind of environment in which effective therapy research can be done. One thinks of the contrast between the outpatient center or counseling center, on the one hand, and the resident institutional center, on the other. The outpatient or counseling center has always labored under certain unavoidable handicaps with regard to the question of controls. Specifically, their clientele is invariably selected because either the client or someone near him thinks therapy is necessary. Thus, the question of motivation for therapy is perforce left uncontrolled, and, to some extent, the companion question of personality organization is also left unresolved. The own-control method has been used to offset these problems and to provide controls for motivation and personality organization. This is often the best that can be done, and there is much to be said for own controls. But the method has complications of its own. In its usual form, the method involves a no-therapy wait period for the prospective client. Few people would care to support the notion that motivation remains constant under such conditions. Nevertheless, this has been the closest approximation possible to controls in outpatient settings.

Matters are different in an institutional setting. Incidentally, the word "institutional" should be read broadly to include hospital, school, factory, etc. The total sample is available. It is entirely feasible to select experi-

mental and control groups from the same population and to randomize the factors of personality organization and motivation for therapy. Thus, controls for these crucial and elusive variables may be attained.

This rosy picture oversimplifies the situation. The very closeness of institutional life brings new experimental problems. Where experimental and control individuals are known, irrelevant factors may intrude, such as differential response by nurses and attendants, rater bias, and the like. But these are not new problems, and where they are dealt with adequately, we should expect over the longer term to see significant research in therapy come from institutional settings.

Ends & Page (31) report a study on the effect of client-centered group psychotherapy on hospitalized alcoholic groups. The setting allowed for excellent control conditions which the authors utilized well. Pretherapy personality status and motivation for therapy were kept equivalent by the simple expedient of random assignment of the patients to therapy and control groups. Instruments used were the self-concept Q-sort and the MMPI. One aspect of the design was differential analysis of results according to two different therapy intervals, 15 interviews and 30 interviews.

The data revealed differential change favoring the therapy groups in self-concept sorts and in self-ideal correlations. Doubling the number of interviews resulted in greater change in these dimensions.

With respect to MMPI results, both therapy and control groups showed significant improvement in a number of scales. Differential changes between therapy and control groups turned up only in the Pa scale.

One thus concludes that changes in self-perception or self-report are evident here, but that other measured changes associated with therapy are minimal.

Mann & Mann (69) studied the relative efficacy of role playing and task-oriented study-group activity in producing changes as perceived by group members. Ratings of each other were made by group members on the dimensions of individual prominence, aiding attainment, and sociability. These categories were derived from earlier factor-analytic studies of small-group interaction. Results of the analysis indicated that the task-oriented study group reported significantly greater change than the role-playing group. This result does a bit of violence to some commonly held assumptions about the effective agents of change. If we can assume that the task-oriented group engaged primarily in a limited kind of intellectual-cognitive activity and that the role-playing group had more scope for affective exploration, we might have expected a reverse result.

Kraus (59) reports a study of group therapy distinguished by control group procedures, systematic observation of patient and therapist behavior, and measures of outcome. Measures of group behavior change did not reveal significant results, nor did pre-postpsychiatric ratings. MMPI changes were observed in the neurotic triad (Hs, D, Hy), with decreases

being evident for the experimental group and increases for the control group. The latter results were the only ones which could be considered as significant outcomes of therapy.

Effects of group therapy on psychosomatic illness were described by Igersheimer (50). Eight patients with psychosomatic illness constituted the group. Only one of the patients reported no improvement. The author reports verbatim interview excerpts to portray the way in which patients moved toward acceptance of their illnesses as part of themselves rather than ego-alien phenomena. As another part of the study, Igersheimer reports the evaluations of the medical staff. The psychiatrist studied samples of the transcripts and concluded that changes were evident in expressed anxiety and in manifestation of specific maladaptive character traits. Physical examinations of the somatic status of the patients showed little consistent improvement. The conclusion, thus, was that while attitudinal factors changed, somatic factors did not.

A follow-up study of the effectiveness of group therapy with medical students was reported by Ganzarain *et al.* (35). The original study involved a matched group design. The authors concluded then that the experimental group showed greater decrease in neurotic symptoms and deeper understanding of transference reactions. The follow-up was designed to evaluate the maintenance of the gains over a two-year period, using the same criteria as in the original study. The evidence pointed to the conclusion that the gains persisted very much as in the original study.

#### CHILD THERAPY

The use of appropriate avenues of emotional expression and release has been a particularly challenging problem where verbal means are not possible. Joseph & Heimlich (53) report a study using music therapy for a group of mentally retarded children with whom other procedures had failed. A rating scale was set up for evaluating behavioral change and filled out by staff members. Results for three of the children were reported. The study was evidently intended to be illustrative of the method rather than evaluative, since we do not have any indication of the total number of participants or their ratings. The chief value of the article lies in the detailed description of the therapeutic use of music.

Discussions of child therapy sooner or later get around to the topic of limits. A variety of rationales can be cited for the use of limits, most of them referring either to reality orientation for the child or inner comfort for the therapist. Ginott (37) expands the issue and suggests a number of active values inherent in the limit-setting process. Prominent among them are the value of symbolic rather than direct release, strengthening of ego controls, the issue of ethics and social acceptability, and the maximizing of the therapist's capacity for emphatic attitudes. Ginott illustrates practical applications of limit setting in a way that makes it clear that he knows his way around a play-therapy room.



One of the problems in conceptualizing the process of therapy is that so many of our concepts refer to verbal behavior. This fact makes it hard enough to explain the noncommunicative adult, and even harder to explain therapy with children. In a provocative article on nonverbal behavior in children, Smolen (102) helps point out nonverbal analogs to verbal behavior. The chief implication for therapeutic theory would seem to be that concepts which attempt to explain the therapeutic process must be inclusive enough to encompass nonverbal as well as verbal behavior.

A few years ago, Levitt (61) evaluated the results of 37 studies of child psychotherapy and concluded with the very dim view that little effectiveness had yet been demonstrated. In a very neat scientific switch, Levitt, himself (along with two colleagues), proceeded to do another study of child therapy (63). No luck. In a careful follow-up of a large sample of remainders and defectors in therapy, no differences were apparent between the groups. The authors point out that the therapy was done chiefly by inexperienced therapists in training, and so the results cannot be applied to therapy in general.

Hood-Williams (48) re-examines the data and arguments of the Levitt review on child therapy, and proposes reasons for reconsidering Levitt's conclusions. The Hood-Williams points are as follows: (a) The baseline problem is most difficult. The use of cases who approached a clinic but did not get service brings up psychodynamic complications. Such cases are not true baseline cases. (b) The data indicate that there is a relation between degree of success and the time period in which the studies were reported. Hood-Williams suggests the possibility that child-guidance clinics have been undergoing changes in severity of cases and conceptions of function. (c) Matters such as length of treatment and therapist qualifications are sometimes left obscure.

In a reply to Hood-Williams, Levitt (62) takes up each point in turn and reiterates his view that no evidence of effectiveness has been demonstrated for child therapy. His reasoning is logical and cogent, and seems to leave the evidence about where it was before Hood-Williams' article.

We have had few studies of therapy outcome in residential treatment centers for children. One of the very difficult problems lies in setting up adequate controls. An earlier and more feasible task is systematic observation. An illustration of this task is reported by Newman (76). The sample consisted of six boys who were housed initially in the Clinical Center of the National Institutes of Health; the study was carried out under the direction of Redl. The method of the study consisted of analysis and rating of daily anecdotal records kept for each child. A classification system and scale were set up to include aspects of self, relationship, and school. Independent judgments of scale placement for the incidents indicated significant movement for five of the six children from early to late in their stay at the center. This is a useful beginning in a little-explored aspect of therapy.

Studies dealing directly with the relative efficacy of varying therapeutic methods are not often found. Phillips (82) considers this question in a comparison of "depth" and "nondepth" parent-child therapy. While we do not have a detailed delineation of the differences between the methods, we may assume that the nondepth method consisted of relatively short-term therapy emphasizing common-sense explanations of the meaning of the child's behavior. The outcome criterion was a questionnaire-type rating scale sent to the parents at a time up to five years after the end of therapy. Phillips reports that parents rated nondepth results consistently ahead of depth results.

One of the puzzling issues about therapy relates to those clients who initiate requests for therapy but do not follow through. One major attrition point comes after the initial contact, at which point some clients never return. Ginott, Blek & Barnes (38) report on the reasons given by parents for nonreturn to a child guidance clinic. The reasons, of course, varied, but a large proportion (about 40 per cent) revolved about either symptom alleviation or the parents' attitude change toward the problem. The other large categories (35 per cent) seem obviously superficial in nature—i.e., difficulty in getting to the clinic or forgetting appointment times. Taken together, the reasons suggest a pervasive ambivalence about use of clinic services. It may be that early recognition of this ambivalence would free more parents to continue seeking help.

#### COMMENTS

Research in psychotherapy is a long-term activity, and the literature of a single year does not always give a clear picture of trends. One way of discerning these trends more clearly would be to analyze the chapters on therapy through back numbers of this *Annual Review*. No extensive analysis will be made here. However, an examination of the number of research articles cited in these back chapters reveals that, taken over a whole decade, the research productivity increase is striking. The sharpest relative gains were made in the time span 1949 (eight articles cited) to 1955 (40 articles cited). Since then, we have had an irregular rise of decreasing proportion. Nevertheless, it seems safe to assume that therapy research is by now a solidly entrenched part of the psychological scene.

What has been the nature of that research during the past year? From the review, one gets an increasing realization that a great many very different kinds of questions can be explored. Note the diversity of research problems covered in this review: selective factors in acceptance for therapy and in continuance in therapy; qualities of the therapist; the therapist-client interaction; changes in client verbalization over therapy; test concomitants of therapy; the larger therapeutic environment. And there are more. Thus, one sees a research domain characterized by variegated and, perhaps, diffuse activity. This is quite likely as it should be. But it should

help dispel the notion that some single research package is likely to be devised to answer a great many questions all at once. The pattern seems rather to be one of plugging away at small bits of knowledge which, only after an appreciable period of time, might attain a high order of significance.

This theme is borne out by the year's research. One notes continued exploration but not advance of a striking sort. Perhaps the most promising single development this year is the more penetrating analysis of client-therapist interaction produced by the studies in experimental psychotherapy. Their microcosmic analyses of the conditions of interaction provide one of the bases for progressively keener understanding of the nature of psychotherapy.

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## CLASSIFICATION OF THE BEHAVIOR DISORDERS<sup>1,2</sup>

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A field of study often has its beginnings in efforts at classification as a means of providing simple and parsimonious ways of comprehending large and varied masses of data. Some investigators, such as Eysenck and Cattell, believe that in psychology, because of the conflict of numerous terminologies and systems, development of a nosological system should actually precede the study of functional relationships. They argue that meaningful research requires the isolation and identification of basic variables. Regardless of how one conceives of the proper approach to an understanding of the behavior disorders, classification has an important, although limited, role to play in this field. In this chapter we shall emphasize theoretical formulations, experimental and statistical studies, and allied research broadly concerned with the origin, development, and classification of the behavior disorders. Biochemical theories and experiments were reviewed by Hoffer in the 1960 *Annual Review* and will not be touched on here. Evaluative studies of psychotherapy are reviewed in another chapter. Reference will be made to drug studies and the shock therapies only in so far as they reflect on or represent applications of theory to treatment. Broadly speaking, the approach taken was selective. Only a fraction of the studies reviewed will be described.

### THE PROBLEM OF DIAGNOSIS

In a provocative article on the validation of clinical procedures Meehl (48) presents a spirited defense of formal psychiatric diagnosis. The basic argument for the utility of formal diagnosis is that there is sufficient etiological and prognostic homogeneity among patients belonging to a given diagnostic group that assignment of a patient to the group has implications which it is clinically unsound to ignore. The Kraepelinian nomenclature remains because a considerable amount of truth is contained in the system. Meehl also makes the point that syndrome description often constitutes our only direct knowledge of a disorder in the absence of known etiology. The value of current diagnostic categories for their relevance to the practical problems of decision making should be considered. Against the argument that diagnostic categories are not dynamic, Meehl suggests,

<sup>1</sup>The survey of the literature pertaining to this review was concluded in April, 1960.

<sup>2</sup>Abbreviations used in this chapter include: MMPI (Minnesota Multiphasic Personality Inventory).

<sup>3</sup>The assistance of Mr. Frederick L. Klein in identifying references is gratefully acknowledged.

as others have, that a similar complaint can be advanced with regard to any disease prior to the explication of its pathology and etiology. To those who object to current nosological schemes because of the low agreement among psychiatrists, Meehl answers that much of this is untrue or attributable to diagnostic error. Multiple indicators treated by configural methods must be utilized before one can decide whether to treat interobserver disagreement as showing the unreality of a taxonomy. Kerbikov (35) also comes to the defense of the Kraepelinian system and asserts that many of Kraepelin's ideas have proved practical and productive.

Conceptualization of the classification process has been clouded for some time now by a confusion between two different concepts of the syndrome or diagnostic category. The older and more conventional class model undoubtedly had its origin in notions about physical diseases, each resulting from a specific pathogenic agent. The class model requires that all or nearly all symptoms or signs be present before the disease can be said to be present. The disease does not vary in amount but is either present or absent. However, unlike physical disease, psychiatric categories are mutually exclusive. A person cannot be classified, for example, both as agitated depressive and as obsessive-compulsive. In short, the class concept of syndrome is typological. A syndrome is a class of patients.

The newer quantitative model has more typically been utilized by psychologists and by those who apply factor analysis to psychopathological data. The syndrome is conceived of as a group of symptoms and signs that characterize a disorder or disease, and the number of manifestations is a direct measure of the amount or intensity of the syndrome. This model thus assumes that the syndrome is present in a lesser or greater degree in all patients.

Dissatisfaction with current diagnostic nosology arises from the lack of agreement between judges and from the multitude of mixed classifications. The low order of agreement between judges as to how a patient ought properly be classified may be due to diagnostic error rather than to the model or to a defect in the syndrome. More importantly, however, the difficulties of classification may result from imposition of a typological (class) approach on a nontypological system. The evidence from the factor analysis of symptoms and deviant behavior tends to support the notion that the so-called psychiatric syndromes operate independently and, in a real sense, are present in all persons to some degree. All patients may be to some degree, depressed, suspicious, or disorganized in their thinking. Therefore, when a psychiatrist tries to diagnose and classify a patient by standard nomenclature, he finds that the patient exhibits two or more deviant tendencies. The dilemma is avoided if, instead, a polydimensional approach to the classification of patients is utilized. Configurations of first-order symptom patterns can then be utilized to describe classes of patients in the

Kraepelinian fashion. This approach would represent a compromise between the quantitative and typological approaches. As yet it is not clear which model provides the better fit to the facts of mental illness. Guttman's circumplex and simplex models for ordering data deserve trial. It could well be that different structural models are required to fit the actual relations obtaining between different kinds of symptoms.

In the Meehl article just described investigators are challenged to cite a consistent body of published evidence showing that predictions based on psychodynamics or "the structure and dynamics of an individual's motivations" are superior to predictions based on information of an actuarial type. Bindra (12) goes further and argues that three decades of psychodynamically oriented research concerned with conscious and unconscious wishes and anxieties as determiners of behavior "have failed to contribute significantly to the problems of causation of psychopathology, diagnosis, and treatment of behavior disorders." He suggests, instead, concentration on observed behavior. For him the crucial problem is that of determining the laws that govern the interaction between habit strength and other factors that control the occurrence of responses. Meehl's statement, if correct, refers principally to decision making whereas Bindra's polemic is broader. The latter's approach seems excessive but may serve as a check to excessive claims made by the dynamically oriented.

*Measuring tools.*—During the year a number of studies were directed at the development of rating scales and check sheets for recording psychiatric symptoms. All except one assumed, implicitly or explicitly, the quantitative model. Jenkins, Stauffacher & Hester (31) report the construction of a symptom rating scale for recording the symptoms of psychotic patients following a careful interview. In addition to its usefulness as an indicator of the presence, kind, and degree of psychopathology, a number of scales have been demonstrated to have prognostic significance. Unfortunately, no normative data are provided. The scale purports to measure certain symptom factors previously identified. Bostian *et al.* (13) undertook the development of an objective quantified mental-status examination. Some 60 status variables were constructed and tried out. The rating scales were subsequently used in a factor analysis to be described later. No data concerning interobserver agreement are provided. Venables & O'Connor (70) developed a four-item scale, for rating paranoid schizophrenia, based on several factor analyses of scales drawn from the Multidimensional Scale for Rating Psychotic Patients. Hamilton (26) devised a 17-variable scale for use only on patients already diagnosed as suffering from affective disorders of the depressive type. Its purported use is for quantifying the results of an interview. The correlation between summed scores for two raters describing 70 patients is .90. The instrument has been carefully developed and deserves more than ordinary attention.

## SCHIZOPHRENIA

*Process vs. reactive schizophrenia.*—A symposium on process-reactive schizophrenia, given at the American Psychological Association Convention in 1958, resulted in four interesting papers next reviewed here. Kantor & Winder (33) hypothesize that a fruitful empirical approach to the study of schizophrenia may be found in conceptualizing the latter as a continuum. Steps on the continuum reflect the stage in the patient's development: empathic, prototaxic, parataxic, autistic, or syntactic. These stages have been made the basis of a life-history rating procedure. The history ratings may then be correlated with rated stage of schizophrenia. Zimet & Fine (20, 76) utilize the framework of process-reactive schizophrenia to evaluate levels of perceptual organization based on Rorschach developmental scores. A study of 36 process and 24 reactive schizophrenics indicated that the groups react to a perceptual task in predicted directions. The process group was found to have more immature, regressive perceptions. The authors wisely conclude that it is not clear whether two different kinds of disease processes, or levels of personality organization, are demonstrated.

Becker (9) takes a related approach in regarding process and reactive syndromes in schizophrenia as end points of a continuum of severity of illness and as reflecting levels of personality organization. Becker made a study of 51 hospitalized schizophrenics rated on the Elgin Prognostic Scale and tested on the Rorschach. The Rorschach Genetic Level score correlated  $-.60$  with the Elgin Scale for 24 males, and  $-.68$  for 27 females. A proverbs test correlated  $-.68$  with the Elgin Scale for the males but only  $.05$  for the females. A factor analysis of the Prognostic Scale items and Rorschach measures from the first response to each card yielded seven factors. Three of the factors, Schizophrenic Withdrawal, Reality Distortion, and Emotional Rigidity vs. Manifest Anxiety, corresponded to a previous analysis by Lorr. The Rorschach variable loaded principally on the first two named factors. Scores based on various combinations of these variables are suggested as good measures of severity of psychosis.

Garmezy & Rodnick (22) take a skeptical view of schizophrenia as an endogenous-process and exogenous-reactive dichotomy. They prefer premorbid adequacy and prognosis as a basis of study and prefer Philips' Scale of Premorbid Adjustment. A series of studies are then reported which indicate that the patients with poor premorbid adjustment have more intensely disturbed relationships with their mothers. On the semantic differential, poor premorbid patients, compared to good, rate concepts of rejection, domination, and punishment significantly more potent and active. Other studies suggest that the poor premorbid group had markedly dominant mothers and submissive fathers, whereas the father of the good premorbid patient was strongly ascendant.

In a study of motivational differences between process and reactive schizophrenics and normals, Reisman (61) instructed his subjects to sort

playing cards at whatever speed they wanted under four conditions. Subjects saw magazine photos judged to represent areas of conflict, frustration, and threat for brief exposures if they sorted fast; if they sorted slowly, the exposures were longer. It was hypothesized that the pictures would be negatively reinforcing for reactive patients who would sort at a tempo which would enable them to avoid seeing the pictures, whereas the process patients would not be motivated to sort rapidly. The authors conclude that there is a deficit in motivation in the process group. McDonough (45) conducted a study to determine whether organicity is involved in process schizophrenia. He hypothesized that process schizophrenics would perform similarly to brain-damaged patients on critical flicker frequency tests and on the Archimedes Spiral. A comparison of normals, brain-injured, process, and reactive schizophrenics demonstrated only that the brain-injured patient differed from the others.

These papers suggest that Garnezy & Rodnick's cautious view of the process-reactive dichotomy is soundest at present. A conservative and more parsimonious view than the process-reactive hypothesis is that there exists a continuum of premorbid adjustment along each of several dimensions which differentiates patients classified as schizophrenics. As yet no satisfactory evidence has been presented that process and relative types exist. A discriminant function analysis would be particularly useful in understanding this issue.

*Experimental studies.*—During the year a number of studies concerned with the schizophrenic perceptual process appeared. Weckowicz & Blewett (72) studied size constancy and abstract thinking in schizophrenia. Hozier (29) sought to explore one of the implications of Des Laurier's theoretical postulates regarding conditions necessary for the development of a stable sense of reality. If it is assumed that the psychological model of reality is the experience of one's bodily self as differentiated, bounded, and separated from everything that is not self, then the loss of sense of reality involves a breakdown in the bodily self as a consequence of insufficient cathexis of the body. To test certain implications of this theory 25 hospitalized schizophrenic and 25 nonhospitalized normal women were administered three tasks: figure placement, doll assembly, and Draw-a-Person. All involved active dealing with spatial relations. As predicted, the schizophrenic group made significantly more errors and were more variable than the controls. In the figure placement test figures were placed in deviant postures relative to a scene. On the Draw-a-Person Test schizophrenics omitted various portions of the figure and distorted others. The results are indeed consistent with the theory proposed. However, much of the findings could have been anticipated independently of the theory.

In response to a number of conflicting reports Rosenthal *et al.* (62) completed a meticulous study of the relation of mental set to the degree of schizophrenic disorganization. The position of one of the authors, Shakow,



has been that schizophrenic patients do not attain as high a level of set or preparation for response as do normal subjects and that they are unable to maintain a set. The experimental subjects consisted of 13 white, male schizophrenics. The criterion of mental health was "ego-intactness" or degree of intelligibility to others, seeming ability to understand what is said, appropriateness of affect, and ability to participate in work or play. Motor response was measured under two conditions of set. It was found that the patients tended to show a loss of set most consistently when the preparatory intervals were increased from 4.0 to 7.5 seconds. The Mental Health index correlated .89 with the set index. Progressive Matrices failed to correlate significantly with Mental Health.

The body image and self-concept of schizophrenics was examined by Weckowicz & Sommer (73). Schizophrenic and nonschizophrenic psychiatric patients and controls were compared with respect to their responses on a variety of tasks including reactions to three panel mirrors, Draw-a-Person Test, and drawing missing parts. Compared to controls, schizophrenics underestimated the size of the distal parts and used self-reference less frequently. The authors feel that lack of self-reference indicates a more limited self-concept. They conclude that in schizophrenics, constancy of perception is poor, objects are poorly defined, and social roles are ill-defined.

By means of a clinical interview, Schofield & Balian (65) compared the life histories of 150 normal subjects in a general hospital with those of 178 hospitalized schizophrenics. The groups were matched for age, sex, and marital status. The schizophrenic group was characterized by higher incidence of unfavorable relations with parents. They exhibited poorer attitudes toward achievement in school, less occupational success and satisfaction, narrower interests, more limited aspirations, vague life plans, and lack of initiative. However, the extent of overlap calls for reservation in the interpretation of any single set of circumstances as contributing to the etiology of schizophrenia. The authors suggest that unknown suppressive and minimizing experiences may play a role. In a related study, Lane & Singer (39) sought to determine what differences obtain between paranoid schizophrenics and normals in attitude toward parental figures. Forty-eight paranoid schizophrenic and 48 normal white males were tested with the Elias Family Opinion Survey and a specially constructed family attitude scale, involving ratings of pictures of family scenes. Schizophrenics showed greater tendency to describe familial patterns involving greater rejection, maternal dependence, direct hostility towards parents, and parental friction. Social class differences suggest greater idealization of the mother and greater rejection of the father.

Milgram (50) studied cognitive and empathic factors in role taking by schizophrenics and brain-damaged patients. The two groups were administered four multiple-choice word-association tests requiring shifts from male to female role and from adult to child role. When role-taking scores were

adjusted for differences in cognitive ability, the brain-damaged group was superior to the schizophrenics on male-female polarity chiefly because the schizophrenic was unable to assume the male role. While both groups were deficient compared to normals, schizophrenics were specifically deficient empathically.

The moral judgment of schizophrenics was the subject of a study by Johnson (32). Since schizophrenia represents an impairment of social understanding and participation, it seemed of interest to study the moral judgment of such individuals. The ability to feel guilty, to be self-critical, or to behave responsibly is much a matter of internalizing the moral values of society. A semistructured interview, based on Baruk's Test of Moral Judgment, was administered to 15 normal subjects and to three groups of 15 schizophrenics: a newly hospitalized group, a predischARGE group, and a chronic group. The results indicated that the moral judgments of schizophrenics were significantly different from normals and varied with the extent to which the patients were "out of society." Normals made predominantly humanitarian judgments whereas schizophrenics placed more importance on self-protective, authoritarian, utilitarian, and common practice values. These reflect social detachment, egocentric self-concern, and a mechanical view of interpersonal relations.

In a study by Lewis *et al.* (40), schizophrenics were shown to do more poorly on proverb interpretation tasks than normal controls. Furthermore, when the content of proverbs is subdivided into oral, anal, phallic, and undifferentiated, schizophrenics perform most poorly on oral proverbs. The results are interpreted as supporting a psychogenic theory of schizophrenia.

*Genetics and culture.*—In a carefully reasoned review of genetic factors in schizophrenia, Gregory (24) casts doubt on the arguments and evidence currently advanced. He argues that there is considerable doubt concerning the significance of genetic factors in the etiology of "functional" psychiatric disorders. It is true that schizophrenia has been studied extensively by means of twin and family data, but the extent and nature of genetic factors remain uncertain. Although twin studies suggest the importance of the hereditary variable, there are serious methodological problems. The frequency of schizophrenia is significantly greater in families of schizophrenics than in the general population, but evidence that frequencies in different classes of relatives do not conform to that expected in simple Mendelian dominance or recessivity is presented. Gregory concludes that no monogenic hypothesis is compatible with all the recorded data. Polygenic inheritance is most likely. Ambrumova (2), in a study of 1000 schizophrenics, found it possible to establish a hereditary loading in 198 individuals. It appeared that, though there was a greater similarity in the initial symptoms, there was less similarity as regards the subsequent course of the psychosis between parents and their children than between brothers and sisters.

Opler (54) suggests that investigators take a look at cultural elements

in research on schizophrenia. Thirty Irish and 30 Italian patients were matched for age, education, and IQ. The groups were distinguished on seven variables as expected by hypothesis. The Irish exhibited more latent homosexual tendencies, more preoccupation with sin and with guilt over sex. They were more compliant to authority, and had fixed delusional systems. On the other hand, the Italians were more likely to show overt homosexual tendencies, but they exhibited no sex guilt, and no fixed delusions. However, they had many somatic complaints. The Irish favored fantasy and withdrawal as defenses while the Italians suffered from a lack of impulse control.

#### AFFECTIVE DISORDERS

Perhaps no psychosis has a better claim to a biochemical basis than manic-depressive psychosis. Biochemical and physiological studies of this disorder have been increasing, and one of great interest is reported by Shinfuku *et al.* (66). The report is based on a five-year study of the correlation of mood with bodily changes of a 50-year-old female patient, IQ 55, who experienced a regular 21-day cycle of overactivity and depressive stupor. Periodic measures were taken of arterial blood pressure, eosinophil count, urine volume, cholesterol, etc. The manic phase was characterized by polyuria, eosinopenia, slight hyperglycemia, low blood pressure, slight hypercholesterinemia, and high estrogen. The depressive phase was marked by oligouria, eosinophilia, high blood pressure, considerable hypercholesterinemia, and low estrogen. The case is considered an example of a disorder in which weakness in the regulation of sexual hormones plays an important role.

Pokorny (57) made a survey of 44 known suicides and compared them with a control group of 44 cases. Thirty-one of the 44 suicides had made previous attempts or threats of suicide. There were also signs and symptoms of depression and remarks made to the psychiatrist indicative of a decision to die. In a carefully designed and executed study of depressive states, Hamilton & White (27) classified 64 patients on etiological grounds into four groups: Endogenous, Doubtful Endogenous, Doubtful Reactive, and Reactive. The four groups differed significantly in their scores on certain symptoms, thus confirming that endogenous depression is of the retarded type. The reactive group appeared not to be distinguished in any special way. Hamilton also claims identification of a psychopathic depression, but examination of the case histories reveals little suggestive of psychopathy as ordinarily defined.

#### NEUROTIC AND PSYCHOSOMATIC DISORDERS

Perhaps because of greater interest in schizophrenia, relatively few theoretical or experimental advances were reported in the literature on psychosomatic and neurotic patients. Ziegler, Imboden & Meyer (75) completed a detailed clinical appraisal of 134 consecutive patients who exhibited

one or more somatic symptoms without apparent anatomical or physiological basis. Additional criteria for hysteria were a psychic basis for symptoms and temporal association of complaints with significant life events. The major symptom groups were classical loss of function, simulation of known organic illness, and pain. Vague pain is the conversion symptom most often encountered. The reactions are interpreted as enabling patients to avoid or reduce affective distress by substituting somatic distress. Basically the hysteric's character is histrionic, but he plays an unconvincing role.

Wahler (71) examined hostility and aversion for expressing hostility in neurotics and controls. According to Freudian theory, the superego in neurotics exercises an overly severe suppressive effect on socially disapproved impulses such as sex and hostility. Mowrer, on the other hand, contends that the superego in neurotics is not severe enough. To test these alternatives, Wahler required neurotics and control subjects to construct and report a sentence from scrambled words so chosen that a hostile or neutral sentence could be written. If the superego were stronger in neurotics, they should select fewer hostile alternatives than the controls. If the superego were weaker, then neurotics should select more hostile sentences. Tests showed that both neurotics and controls selected fewer socially disapproved statements than approved statements. However, when the statements were controlled for human content, the neurotics selected more hostile statements toward humans whether approved or disapproved. Klaber (36) designed a study to get evidence that neurodermatitis patients suffer from suppression of intense hostility. One hypothesis was that such patients would evidence no more overt hostility than control subjects suffering from nonpsychosomatic skin diseases. The second hypothesis states that neurodermatitis patients would manifest more covert hostility. The tests consisted of the Thematic Apperception Test and a scale of manifest hostility. Klaber concludes that neurodermatitis patients are generally hostile individuals who expend great efforts on successful control of impulses. Outwardly they appear calm and controlled.

In an effort to determine the correlates of acute bronchial asthma, Knapp & Nemetz (37) studied 406 attacks in nine patients receiving psychotherapy. The indicators were classified as to feelings accompanying attacks, prodromal phenomena, and antecedent events. In 45 per cent of attacks depressed feelings were reported. In one-third of attacks angry-anxious feelings preceded the attack. The antecedent events appeared to be the threatened loss of a person (27 per cent), ego threat (7 per cent), loss of a substance (10 per cent). The authors conclude that the Alexander-French hypothesis that asthma represents a "suppressed cry for the mother" is not supported.

Vandenberg (69) reanalyzed data initially reported by Eysenck on differences obtaining between 20 neurotics, 20 psychotics, and 106 normals on a lengthy battery of perceptual tests yielding 91 scores. Eysenck con-

tends that neuroticism and psychoticism form two orthogonal dimensions of personality. If this view is correct, we would expect that the means of the three groups would not always be in the order: normal, neurotic, psychotic. Vandenburg found that on 41 of the 81 *t*-tests the neurotics did not occupy an intermediate position between normals and psychotics. However, of 91 scores only seven discriminated between neurotics and psychotics at the 5 per cent level. Only one test, critical flicker fusion, discriminated all three types of persons. The results are interpreted as tending to support Eysenck's bifactor theory of neurosis vs. psychosis.

#### DELINQUENT AND PSYCHOPATHIC BEHAVIOR

The publication of *Adolescent Aggression* by Bandura & Walters (7) represents a considerable advance in the theory of juvenile delinquency. As Sears points out in his foreword, the theory had its origins both in psychoanalytic observation and in learning theory concerned with rewarding and punishing. The first provided the theory of motivation and the second the theory of process by means of which the motivational variables are made operative. In brief, the book is an account of an empirical study designed to identify child-training practices and family interrelationships that contribute to the development of antisocial behavior in adolescent boys. The method consisted of semistructured interviews. Twenty-six adolescent controls and their parents and 26 boys with a history of antisocial behavior and their parents were contrasted. All subjects selected were of above average intelligence, between 14.5 and 17.9 in age, and from legally intact homes. Families were paired for occupational status and for the boys' age. Ratings were made on 61 scales from recorded interviews by advanced psychology graduate students. It was assumed that a primary condition for socialization is development of a dependency motive. Frustration of the child's dependency needs, through a lack of affectional nurturance on the part of one or both parents, provides the condition for generalized antisocial aggression. Studies were made of dependency; aggression; sex behavior; the influence of restrictions, demands, and disciplinary techniques; and, finally, of internalization of controls. The majority of hypotheses were supported. The portrait of the aggressive adolescent that emerged was that of a person who expresses his aggression in a direct and uninhibited manner. Compared to the controls he is openly antagonistic to authority and less positive towards his peers. He feels rejected by both parents but less so by his mother; he is especially critical and resentful of his father. His aggressions are held in check primarily by external restraints. By and large he is mistrustful of adults in authority.

Herskovitz, Levine & Spivack (28) studied the antisocial behavior of adolescents from a higher income group in hope of gaining insights on etiology. The records of 55 males with previous history of delinquency were compared with those of 50 control subjects; both groups were enrolled in the Devereux School. The groups were matched for age, socio-

economic background, and the like. Their findings, similar to those described by Bandura & Walters, suggest that factors making for delinquency are similar irrespective of income level or the physical adequacy of the home.

A factor analysis by Peterson, Quay & Cameron (55) of questionnaire data descriptive of psychopathic and delinquent tendencies is described elsewhere in this chapter, but it should be observed here that on the whole their findings tend to be consistent with the studies just cited. The nonfactorial studies are obviously more illuminating of the origins of adolescent aggression and delinquency. However, the factored questionnaires are probably more useful as devices for making decisions about treatment. The theory presented also suggests the potential usefulness of measures of dependency, attitudes towards discipline, and the like in future research.

Tong (68) examined stress reactivity in relation to delinquent and psychopathic behavior. Male adults were examined by means of four techniques for assessing overt and autonomic responses to stressors. The stressors consisted of heat, brush on the eyelids, and frustration tasks. The delinquent subjects fell at one or the other extreme of reactivity. The controls manifested moderate reactivity.

#### FACTOR STUDIES OF PSYCHOPATHOLOGY

Factor analysis as a method has now been available for some 25 years. Virtually all major problems relating to the procedure, such as estimating communalities, determining the number of factors, and rotating to simple structure analytically, have been solved. Is the procedure worth while in the domain of psychopathology? Its use for the analysis of ratings of psychotic behavior does appear to have been fruitful. Isolated symptom patterns have been repeatedly confirmed in different patient samples, and the patterns change meaningfully as a result of drug treatment. However, the application of the method to questionnaire and objective test data is yet dubious. What follows will be seen to support this statement.

Martin (47) administered to a group of 98 college women a test battery of 37 measures which previous research had suggested involved, in some degree, individual differences in anxiety. Among the tests included were easy and hard paired associates, various motor tasks, the Manifest Anxiety Scale, defensiveness scales, check lists of worries and annoyances, and the American Council on Education Psychological Examination. All except two of the nine orthogonally rotated factors were defined by clusters of similar tasks. Only one factor of anxiety was identified by the author, although another factor was defined by all of the paper and pencil check lists and questionnaire. No reference variables to represent dimensions isolated in earlier studies by Mosier, Guilford, and others were included. Inasmuch as the anxiety component of most of the tests was of a doubtful order, and the check lists were developed as indices of neuroticism, it is no surprise that so little emerged that could be labelled anxiety.



Scheier, Cattell & Horn (64) described a study of their neuroticism factor Universal Index (UI) 23, one of the 20 factor dimensions discovered among objective behavioral-personality tests. Two related studies checked and improved the measurement of this dimension by modifying existing tests and developing new ones. One of the studies also correlated score on the factor with a category of psychoneurotic vs. normalcy. The correlation of .27 suggests that UI 23 may be only one of several dimensions of neuroticism. UI 23 appears to be most similar to neurasthenia and is also related to Eysenck's neuroticism factor.

In another study, Cattell & Scheier (16) report efforts to extend the meaning of objective personality factors UI 23 and 24, putative measures of neuroticism and anxiety. A sample of 86 male students, including a group preselected for high anxiety, was administered a battery of 103 variables. Included were 33 questionnaire scales taken from Cattell's 16 Personality Factor (PF) battery, 10 objective factor tests, and three dynamic source traits. Some 17 factors were extracted. Matching was claimed for nine factors. UI 23 was best defined by such measures as higher increase of heart rate after startle, higher mean writing pressure, lower ratio of accuracy to accomplishment, and higher pupil dilation at stress. UI 24, labelled Free Anxiety, was defined by questionnaires as involving less will control, more reported anxiety, more ergic tension, lower saliva volume, more honesty in admitting frailties, less superego strength, etc. It is not clear whether UI 24 is a second order factor or if the 16 PF variables are not as independent as claimed.

Cattell and his associates have directed their research at a complete taxonomy of the human personality. Sixteen factor dimensions have been reported as established and matched in questionnaire and rating media. More recent work claims establishment of some 20 dimensions in objective performance tests. Becker's (8) discussion of the matching of behavior rating (BR) and questionnaire (Q) personality factors throws some light on the enterprise. His review of recent reported studies of factor matching from Cattell's laboratory indicates that there is little support for the claimed congruence of BR and Q factors. One should expect that BR and Q factors would have substantial intercorrelations; they are nonsignificant. Surely BR and Q factors should load on the same factors and no others; they do not. Becker plausibly suggests that cross matching of the two types of factors must take into account defenses that limit self-awareness, differences in frames of reference, and differences in experience.

Hamilton (26) constructed a 17-variable rating scale for depression. The intercorrelations between the scales were factored by the method of principal components with unity in the diagonals. Six factors were extracted but only three were rotated. Dissatisfied with both the rotated and unrotated factors, which appeared not to correspond to the classical clinical syndromes, Hamilton concluded that the technique (factor analysis) is

really incapable of demonstrating them. The clinical syndromes, he contends, are mutually exclusive, i.e., a patient can be ill from a reactive depression or from schizophrenia but not from both. The model just described is, of course, the classical model of physical disease. However, Hamilton's test of the factor analysis model is somewhat less than adequate. The use of unities in the correlation matrix, rejection of four latent vectors without statistical tests, failure to achieve simple structure, and a lack of marker variables from the seven known factors in this area scarcely provide the evidence to support his belief that the factor-analytic approach is inappropriate.

Lingoes (44) sought to test Wiener's contention that items of the MMPI could be separated into two factors (subtle and obvious items) against Harris's multifactor position. The sample included inpatients, outpatients, and normals. From eight Wiener and 28 Harris subscales analyzed in four samples, seven factors were replicated in four analyses and another four factors in three analyses. The factors identified were General Maladjustment, Denial of Social Anxiety, Loss of Control, Denial of Distrust and Hostility, Family Discord, Inhibition and Apathy, Social Nonconformity, Hostility-Alienation-Projection, and Somatic Complaints. Lingoes concluded that the MMPI can measure more than two dimensions.

Bendig (10) attempted to determine the number of orthogonal factors that could account for scores in the measures of anxiety, neuroticism, and introversion-extroversion developed by various workers. He postulated a "general" factor of Emotionality. Some 425 college students, split into two samples, were tested on a battery of 13 measures including the Taylor Anxiety Scale, Winne's Neuroticism, Edwards' Social Desirability Scale, Eysenck's E-I Scale, Eysenck's Neuroticism Scale, Cattell's Covert and Overt Anxiety, and Cattell's Neuroticism Scale. The MMPI Lie Scale and Cattell's Motivation Distortion Scale and the subjects' sex were also included. The three orthogonal factors emerging were deemed measures of Emotionality, Femininity, and Falsification. A second study was then conducted to permit the appearance of Extroversion-Introversion (E-I) which had been represented by only one test. Three measures of E-I and two measures of neuroticism were obtained by grouping items from Eysenck's Maudsley Personality Inventory into independent scales. The expected E-I factor now appeared along with Emotionality, Falsification, and Femininity, the latter being defined by Cattell's Neuroticism Scale. The use of broad global scores containing a hodgepodge of items and the absence of marker variables to represent other known factors in the domain make it difficult to see what can be concluded from such a study. True, the common-factor space has been accounted for as Bendig claims, but not all the common factors have. Perhaps Emotionality is a term to be preferred to Neuroticism or Anxiety.

Kassebaum, Couch & Slater (34) factored 32 MMPI scale scores ob-

tained from 160 college freshmen. Their analyses yielded two bipolar factors, interpreted as Ego Weakness vs. Ego Strength and Introversion-Extroversion, and a third doubtful factor. Since Welch's Anxiety (A) and Repression (R) scales defined the two factors with virtually no overlap, the parameters can be understood by examining these two scales. Welch points out that all items, save one, on scale A are answered True and every item in R is answered False. However, this is not a simple response set since the two yield a zero correlation. The authors contend their findings confirm Eysenck's results and Welch's, as well as eight factorial studies of the original MMPI scales, even though conceptualizations have differed from study to study. It is quite evident that the two factors emerging from these studies are second order in nature, but their significance will remain in doubt until some clearer understanding is obtained of the primary characteristic being measured. What is the nature of Welch's A and R factors? There could be two independent "yea-saying" tendencies represented here. Messick (48a) factored a social desirability scale and found about 10 factors. Comrey's analyses suggest that the "yea-saying" scales could also be split into numerous subscales.

Peterson, Quay & Cameron (55) administered a combined form of two previously developed "delinquency" scales to 116 delinquents and 115 non-delinquents matched for age, place of residence, and race. Fifteen factors were extracted from the matrix of phi coefficients and five orthogonal factors were interpreted. The first factor, interpreted as Psychopathy, suggests a tough, amoral, rebelliousness coupled with distrust of legal and other authority. The factor is judged to be close to Jenkins's Unsocialized Aggression and Comrey's Psychopathic Personality. The second factor is interpreted as Neuroticism since it includes indications of depression, guilt, remorse, and discouragement. The remaining factors are labelled Family Dissension, Inadequacy, and Scholastic Maladjustment. The last two are doubtful.

Astin (6) obtained MMPI data on a sample of 250 male drug addicts. A factor analysis of the Psychopathic Deviate scale revealed five factors—Self-esteem, Hypersensitivity, Social Maladaptation, Emotional Deprivation, and Impulse Control. Despite differences in the samples involved in the Astin and in the Peterson study, there are similarities in the factors identified. Since both were exploratory studies it would now be of interest to pool the useful items and attempt to confirm some of the factors on a sample representative of both groups.

O'Connor & Stefic (53) obtained MMPI scores on 300 white male, hospitalized psychiatric patients. Four factors were extracted from 30 items of the Hypochondriasis Scale. The oblique factors interpreted were called Asthenic Reaction, Vague Somatic Complaints, Gastrointestinal Reaction. The second order factor of Physical Health or health concern was

then identified. It corresponded closely to Comrey's first general factor.

Sixty rating-scale variables from a rating form designed to represent the mental-status examination were analyzed by Bostian *et al.* (13). Overlapping portions of the correlation matrix were factored and then rotated by the quartimax method. The factors are identified as Psychotic Confusion, Hostile Acting Out, Ego Resources, Activity Level, Somatization, Anxious Remorse, Dominance-Submission, and Compulsivity. A few factors such as Hostile Acting Out and Anxious Remorse are probably identical with behavior rating factors previously identified. However, the results are relatively ambiguous and inconclusive. The study illustrates the need for generating hypotheses prior to construction of the instrument itself and for including markers from well-established factors.

Except, possibly, for the Cattell-Scheier studies, most of the others reviewed failed to satisfy in one or more respects basic requirements for an adequate factor-analysis design. Most of the investigators failed to include marker or reference variables to represent known or established parameters in the domain analyzed. Without such markers it is simply not possible to talk about the independence of a factor with any confidence. Secondly, some of the analyses were made on summary scores for scales involving overlapping items, a procedure which violates the requirement of experimental independence of scores. This error, which was committed by several of the investigators could be avoided by analyzing item responses. Finally, some of the studies were based on limited samples. Unless all known sources of variation are present in the sample studied, generalization to broader groups is hazardous.

On the whole, the results from these factor studies are not impressive. A sound beginning appears to have been made in the area of delinquent and psychopathic behavior. The studies of anxiety and neuroticism are, however, disappointing. The fact that most were exploratory investigations rather than confirmatory factor analyses provides a partial explanation for the inconclusive results. However, lack of rigor in design and failure to develop hypotheses also played a role. It is our impression that these studies would have benefited from the utilization of current theory of psychopathology and personality structure. At the early stages of factorial exploration of an area of interest, the design specialist would gain from meeting with the clinician and with the personality theorist in order to evolve testable hypotheses.

#### THEORETICAL DEVELOPMENTS

Pathological functioning of the nervous system and abnormal behavior may result from information input underload, better known as sensory deprivation. The organism must receive a certain rate of flow of sensory information for normal development, particularly in infancy, and for ad-

justment throughout life. If underload occurs, pathological behavior and possible structural damage may result. Miller (52) asks whether information input overload also can produce psychopathology. Behaving systems can be studied in relation to performance, mechanisms of defense, and costs. In a series of studies, he finds that channel capacities per channel are less the larger the system. Mechanisms of adjustment include omission, error, delay, filtering, multiple channels, and escape from the task. Comparable performance curves apparently hold across systems and reveal similar mobilization of comparable defenses. Miller feels that this approach may, in time, help explain some of the psychopathology of everyday life and clinical practice. We suspect that the study of sensory overload would be more productive if stress, frustration, and threat were added in factorial designs.

Sensory deprivation has been widely used as an approach to the understanding of hallucinations and other pathological states. Experiments in which input was virtually eliminated have produced few hallucinatory experiences, whereas those with a low level of sensory input have induced a greater number of such experiences. Davis, McCourt & Solomon (19) studied 10 college students in a tank-type respirator. A constant repetitive auditory stimulus and a light flashing on at random intervals constituted the main sensory inputs for 10 hours. Some subjects experienced hallucination, mental clouding, and reported somatic complaints. The authors suggest that hallucinations are a result of isolation from meaningful contact with the outside world. Grunebaum, Freedman & Greenblatt (24) studied the relation between ego-integrity and the effects of an eight-hour period of sensory deprivation produced by masking goggles, masking noise, and arm restraints. Subjects reported striking perceptual aberrations which were unrelated to ego-integrity ratings. Sensory deprivation is interpreted as an ambiguous situation that the subject structures and handles with his habitual adaptive resources. The authors conclude that sensory deprivation throws little light on personality dynamics. Ziskind *et al.* (77) reported observations on the mental symptoms of eye-patched patients. The symptoms were similar to those reported by others. In addition, a symptom of non-compliance or the removal of the eye patch contrary to the physician's instruction is reported.

A study of the relation of auditory hallucinations to sound stimuli is reported by Ross (63). Ross's clinical experience suggested, in agreement with Strecker, that an hallucination is a perception involving distortion of a stimulus. It is not, as commonly defined, a perception in the absence of an external stimulus. He hypothesized that auditory hallucinations occur more frequently in the presence of sound stimuli. Five paranoid schizophrenics and two involuntional melancholic patients were tested in a sound cubicle that could provide an ambient noise level. Within a 90-minute period, the patient was exposed to conditions of no noise, below threshold noise,

and above threshold noise. Patients were asked to report whenever they heard anyone saying anything. Only one patient reported hallucinations in absence of audible sound. Apparently, receptor stimulation is required for auditory hallucinations. Ross's findings are similar to those based on sensory deprivation experiments in requiring some sensory input before hallucination takes place.

Knopf & Fager (38) studied differences in gradients of stimulus generalization as a function of psychiatric disorder. The hypothesis tested was that psychotics would show more stimulus generalization than normals or neurotics. The findings supported the belief that severity of psychopathology is related to stimulus generalization. This is consistent with the idea that increasing the strength of drive raises the entire gradient of stimulus generalization, that is, the range of stimuli that will elicit a response.

Feelings of hostility and self-criticism/guilt and expression of extra-punitive and intropunitive attitudes are important in diagnosis and psychopathology. Caine (14) sought to study these related factors and selected 17 melancholics and 14 paranoids as extreme representatives of these tendencies. The measures included scales of acting out hostility, projected hostility, and self-criticism made up from the MMPI items; a scrambled-sentence test with neutral and hostile completions; and eight Thematic Apperception Test (TAT) cards. Also included was a tapping test in which paranoids are known to be high. The paranoid group had significantly higher scores on projection of hostility, and lower scores on self-criticism, higher TAT ratings of hostility, and much higher tapping scores. The study fails to show that guilt or self-criticism may be regarded as introjected hostility.

Wittenborn & Kline (74) report a new procedure for inferring drive strength that purports to have practical relevance for external criteria. Estimates of the strength of drives among mental hospital patients for resisting the requests of superiors, seeking the attention of superiors, and expressing pleasure were found to bear an expected pattern of relationships with the symptom manifestations of these patients as ordered by the use of a psychiatric symptom-rating scale.

One of the most stimulating and encouraging publications to emerge during the year was a collaborative volume, *Inner Conflict and Defense* by Miller & Swanson (51). The book represents a report of a series of empirical studies aimed at testing hypotheses about the social origins and child-rearing practices that predispose children to favor particular methods of resolving conflict. The studies concentrate on two types of conflict: aggression vs. moral needs, ambition vs. fear of failure. The conflicts were studied in relation to moral standards, mechanisms of defense, and differences in expressive style. The assumption was made that moral standards, defenses, and expressive style are learned. Seventh- to ninth-grade school pupils in Detroit, from middle and working classes, and a group of



unmarried male undergraduates from Minnesota constituted the study sample.

Defenses were grouped into two families on the basis of criteria of simplicity, degree of distortion, generality, and degree of difficulty created. Denial in act or in fantasy illustrate the first family of defenses. These require little previous experience and maximum distortion, may be widely applied, and create social difficulties. It was hypothesized that these would be more characteristic of manual laborers than middle-class individuals. The second family includes displacement, projection, reversal, isolation, and turning against the self. These were regarded as middle-class defenses. They require many skills, involve minimal distortion, are specific to a situation, and result in socially acceptable behavior. It is, of course, impossible to describe the findings in any great detail. Broadly speaking, the hypotheses were supported. The defenses do relate to social class, to child-rearing practices, moral standards, and expressive style. This report will undoubtedly stimulate many related and confirmatory experimental studies. The well-established diagnostic groupings would benefit from efforts to relate them to child-training practices and to social class.

#### A NEW HANDBOOK

The year was marked by the appearance of the *American Handbook of Psychiatry* in two volumes. Both were edited by Arieti (3). The work represents an effort on the part of 111 recognized authorities to present the concepts, techniques, problems, and prospects of psychiatry today. An attempt was made to represent all points of view rather than to achieve uniformity or consistency. The first volume consists of seven parts which include a general section followed by sections on the psychoneuroses and allied conditions, the functional psychoses, psychopathic conditions, psychosomatic medicine, childhood and adolescence, and a final section on language, speech, and communication. The second volume presents sections on organic conditions, the various therapies, relations with basic sciences, management and care of patients, and legal psychiatry.

The chapters of interest here are those concerned with descriptions of the various syndromes. Chrzanowski (17) considers neurasthenia and hypochondriasis, its historical origins, and a variety of theoretic conceptions. Abse (1) provides historical perspectives, describes the clinical manifestations of hysteria, and goes on to discuss recent concepts. Friedman (21) stresses the distinctions between phobias and discusses the role of various defense mechanisms in phobias. Portnoy (58) is concerned with anxiety states. Etiological factors, psychodynamics, and broader implications are presented. An unusually clear clinical picture of obsessive behavior and of the psychodynamics involved is given by Rado (59). Gutheil (25) presents a brief chapter on reactive depressions, while Michaels (49) delineates the character disorders with emphasis on psychoanalytic concepts. In general, it is noteworthy how rarely the authors refer to experi-

mental studies, quantitative measures, or data of a statistical nature. The overwhelming majority of references are to clinical reports and theoretical formulations. The approach of the authors, except possibly in the chapter on anxiety states, is to present a wholly verbal account of a clinical condition. Perhaps this is the state of the science, but more rigorous approaches are to be desired. An illuminating discussion of schizophrenia and the manic-depressive psychoses is offered by Arieti (4, 5) in two chapters. Cameron (15) ably summarizes the status of paranoid conditions and paranoia, but very little supporting data of an experimental nature is provided. Bigelow (11) presents a brief description of the involuntional psychosis. The psychopathic states are treated by Cleckley (18). These states are differentiated from other psychiatric conditions and the major diagnostic criteria are listed. There is also a chapter on psychosomatic medicine by Lidz (41). Reiser & Bakst (60) offer a well-documented summary of the psychology of cardiovascular disorders; Lidz & Rubenstein (42) offer a similar chapter on the gastrointestinal disorders. The chapters on psychosomatic medicine are distinguished from the others listed here by the number of supporting references to physiological and experimental studies. The Handbook should satisfy a real need on the part of students and research workers for an authoritative account of the psychiatric syndromes. It is our hope that the next revision will be more firmly based on supporting research without loss of clinical pictures that give concreteness to the student's understanding of the disorders.

#### TRENDS

(a) The process-reactive schizophrenia dichotomy has been revived once again. The concept is much in need of closer examination and may prove to be a blind alley, but it has the virtue of stimulating research. The flood of research on schizophrenia continues and numerous theories have been propounded. Yet it seems all too easy to confirm one's hypotheses. Much needed are crucial experiments that will serve to eliminate some of the contenders.

(b) The area showing greatest promise at present is represented by *Inner Conflict and Defense* (51). Experimental studies of the influence of child-rearing practices and moral standards on conflict and defenses and studies of the defenses themselves represent approaches that are likely to be utilized increasingly.

(c) The number of factor analyses is increasing. Now that any researcher can get a centroid analysis and a varimax rotation completed for him at relatively little expense, correlational tables will be diligently explored. However, it takes more than a table of correlations to produce a study that can contribute to the understanding of the field. Careful confirmatory analyses followed by experimental studies to establish the validity of factors isolated are needed.

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\* English translation will be announced in *Technical Translations*, issued by the Office of Technical Services, U.S. Department of Commerce, and will be made available by the Photoduplication Service, Library of Congress, and by the SLA Translation Center at the John Crerar Library, Chicago, Illinois.

## MOTIVATION AND PERFORMANCE<sup>1,2</sup>

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In his chapter on motivation in the 1959 *Annual Review of Psychology*, Cofer (31, p. 194) concluded "... that, if present trends continue, motivation as a distinctive concept, coordinate to other psychological concepts, may well disappear." The preface to Bindra's book on the same subject, appearing in the same year, contained the statement that "... the area of motivation in current psychology is not defined by any unique physiological or psychological process, but only by a class of phenomena" [Bindra (10, p. iv)]. The present reviewer, while acknowledging as he must the scattered and unsystematized character of the literature and the nearly useless status of the word "motivation," cannot agree with these diagnoses and prognoses. He has attempted to show that at least one motivational concept, that of preference, is exactly co-ordinate with what appears to be an altogether indispensable concept outside of the field of motivation, namely, that of discrimination [Irwin, (57)]; the remainder of this review is offered as evidence that great energy and inventiveness are funneling successfully into investigation of the effects of drive and incentive on behavior, which in itself, is only a small part of the whole field. Perhaps students of motivation merely need to assert themselves more aggressively as friendly competitors of students of learning, physiological psychology, and personality—although systematization, such as is attempted by Bindra in the book just cited, is no doubt a principal need of the field at the present time.

This chapter is intended to cover recent work on the effects upon behavior of varying drive strength and amount of incentive. Since this is not an appropriate place for setting forth new definitions, usage of motivational terms like "drive" and "incentive" will be guided by what seems to be common practice. Stress will be laid upon fact rather than theory, to the extent that the two are distinguishable. Material that has been discussed in recent chapters in the *Annual Review of Psychology*, or in the review of the effects of incentive magnitude by Pubols (93) (which appeared in March, 1960), will usually be referred to only for its relation to later work.

Recent publications containing important discussions of topics related to

<sup>1</sup>The survey of the literature pertaining to this review was concluded in May, 1960.

<sup>2</sup>Abbreviations used in this chapter include: FD (food deprivation); WD (water deprivation); FR (food reward); WR (water reward).

<sup>3</sup>The author wishes to acknowledge with thanks the comments of Dr. Eugene Eisman and Dr. Kenneth P. Goodrich, each of whom read portions of the manuscript of this review during its preparation. He is also grateful to Dr. Robert C. Bolles for helpful correspondence and to Dr. Donald J. Lewis and Dr. Carl P. Duncan for making available for study the data of their experiment (72).



those of this review, but that will not receive special attention here, are various chapters in the *Nebraska Symposium on Motivation* for 1958 and 1959; chapters, largely on theoretical matters, in the first two volumes of Koch (63); the book by Bindra (10) mentioned above; and an article by Young (129). Effects of drive and incentive have been dealt with in a review of secondary reinforcement by Myers (87) and in one on partial reinforcement by Lewis (71).

A number of topics were excluded from review for reasons ranging from practical necessity to little more than the reviewer's preference. Perhaps the most important of these are effects of unquantified verbal instructions as inducers of drive or reward (a few studies in this class were admitted), effects of anxiety as measured by questionnaire devices, and effects of projectively measured motivational states. On the other hand, some experimental work was included because it bore on topics under discussion, even though the author's principal interest was elsewhere.

#### DRIVE AS AN ACTIVATOR

*General discussions.*—The prevailing recent view has distinguished between drive as an activator or energizer and drive stimulus as a cue, but sharp disagreement over the necessity or usefulness of the first of these two concepts has arisen. The lines of combat can be seen by comparing Spence's (106, 107) neo-Hullian treatment of drive and Malmö's (75, 76) "neuro-psychological" case for an activator with the attacks upon the concept by Bolles (14) and Estes (41). Defenders of the notion of drive as an activator are in the always difficult position of having to show that phenomena exist which cannot be handled more parsimoniously by a one-factor theory. It is possible for them to appeal, however, as Malmö does, to developments in physiological psychology such as the activating functions ascribed to the reticular formation by Lindsley (73, 74), Stellar (113), and others. On the other hand, Estes' ingenious attack, undertaken to demonstrate that stimuli can do what drive and drive stimuli have been required to do, is the less convincing the easier it is, since the ease arises largely from the unobservable character of the hypothetical stimuli. As long as both sides play the game with unobservables, crucial experiments are impossible and we shall have to wait for history to have its say.

*Activity and exploration.*—In view of the indecisiveness of much of the literature on the variables that determine activity measures, it is a pleasure to come upon the results of Moskowitz (85). He found a nearly linear increase from about 100 to 1100 revolutions per hour in activity wheels as rats' body weight fell from 85 to 60 per cent of normal. Weight was reduced gradually over a 41-day period by manipulation of food deprivation (FD) and water deprivation (WD), a procedure that Moskowitz believes provides better control over running activity than does a fixed daily ration or FD schedule.

Running-wheel activity was also observed by McDowell & Brown (79), who subjected rats to two four-day periods of total FD separated by 20 days of 23-hr. FD. The measures increased during each of these three periods, but more steeply during total FD. The absolute increase was greater during the second period of total FD than during the first; but, since the curve was at a higher level in the later period, the percentage increases did not differ greatly.

There have been three recent reports on rate of unreinforced bar pressing ("operant level") by rats as a function of FD. The rate increased with FD in the studies of Clayton (30) and Davis (36), but Segal (101) obtained the opposite effect. The reason for the inconsistency is not obvious; perhaps it is in some way related to the fact that Segal controlled drive by way of body weight, whereas Clayton and Davis varied time since last meal.

Rats explored a symmetrical Y-maze less under 23 hr. of FD than under ad lib. feeding in an experiment by Carr *et al.* (28). Such contrary results as those of Adlerstein & Fehrer (1) were attributed to effects of external stimulation. This problem badly needs a major cleanup job; the literature is excessively difficult to evaluate because of the heterogeneity of measures and conditions that have been chosen at times, it seems, almost by caprice.

*Consummatory and instrumental behavior.*—Among the several recent studies of eating and acts that are instrumental toward eating as a function of FD in rats, that of Clark (29) stands out for the beauty of its results. He brought his Ss to about 85 per cent of free-feeding weight and observed each at seven FD periods ranging from 1 to 23 hr. The measure used was frequency of bar pressing under variable-interval reinforcement, the mean reinforcement interval of 1, 2, or 3 min. being a parameter of the experiment. Rate of response was a negatively accelerated function of FD for each mean reinforcement interval and appeared to be close to asymptote at 23 hr. Quite remarkably, the response rates under the three variable-interval schedules were related proportionately over all FD intervals to a very close approximation.

Ehrenfreund (39) reported that food consumption by rats was so highly correlated with the amount of body weight lost during the immediately preceding FD interval that such weight loss may be a better predictor of consumption than is the prefeeding weight. Moll (84) observed that rats with limited eating time consumed progressively more in that time over a 32-day period, and that those with limited amounts of food required progressively less time to consume it. After 10 days, Ss at 80 and 90 per cent of ad lib. weight did not differ on any measure. Birch, Burnstein & Clark (12) recorded rats' depressions of food troughs in their cages under a schedule of 2 hr. of food per day for more than 30 days. The frequency of depressions changed from a nearly uniform distribution over time to a J-shaped function with maximum near feeding time. When the Ss were given four runway trials without pretraining and after 15, 22, 25, or 37 hr. of FD, no sig-

nificant difference appeared on the first trial, but a sharp maximum occurred at 25 hr. on the remaining trials. Rats were trained by Bare (5) to get all of their food by bar pressing and were then subjected to a single FD period of 2, 4, 8, 12, 18, or 24 hr. starting at 7:00 p.m. Eating was then observed over the remainder of a 48-hr. period. The most interesting feature of the results was that the animals did little or nothing to make up for FD, but, rather, gave curves of eating that closely resembled the curves under ad lib. conditions for the same times of day.

As part of a program of comparative studies, Bitterman, Wodinsky & Candland (13) obtained curves from African mouthbreeders (*Tilapia macrocephala*), both for direct feeding and for pellets obtained by striking a target, as functions of FD up to seven days in length. Both curves rose sharply up to two days of FD and then more slowly for the remaining five days. Resemblance to curves obtained by Stellar & Hill (114) for drinking by rats was noted by the authors.

The effects of differential FD during acquisition of running in a runway, during early extinction, and during late extinction were studied by Barry (6). Two values of FD,  $2\frac{1}{2}$  and  $26\frac{1}{2}$  hr., were employed in a factorial design in which half of each group was switched in FD at the beginning of extinction and half of each of these subgroups was switched later in extinction. Running speeds were higher for the Ss under high FD during each of the three periods. High FD during acquisition produced faster running during early extinction, the difference diminishing during late extinction; high FD during early extinction led to faster running in late extinction, the difference diminishing to zero. Comparing Ss with changed against Ss with unchanged FD, the former ran slower in early extinction and faster in late extinction, a finding that invites speculation.

A comparison of 23 hr. of FD with FD having the same mean value but varying over 18, 23, and 28 hr. led to the conclusions that, although final starting speeds in a runway did not differ between groups of rats under these two conditions, the variable FD led to an apparently higher asymptote of running speed [Wike & Barrientos (122)]. The result seems not to have been produced by differential weight loss. A program designed to compare the effects of variable drive with those of variable incentive might go far toward improving our understanding of both processes. A beginning has been made by Wike, Kintsch & Gutekunst (123) in their study of the effects of constant vs. variable amounts of predrinking and water reward (WR) upon the running speed of rats in a runway. According to the authors, neither of these differences affected acquisition (although it appears that their analysis of variance failed to detect what was fairly clearly a reliable superiority of the variable-drive group over the constant-drive group), but during extinction under constant drive the Ss that had had variable drive or variable WR in acquisition ran faster than those that had had constant drive or WR.

It has become increasingly clear during the past few years that FD, defined as the time between the S's latest meal and the beginning of the experimental period, is not generally an optimal experimental variable, since measures of performance can vary materially with the properties of the over-all deprivation schedule even when FD, so defined, is held constant. This point was made by Eisman (40), who also (correctly, we believe) suggested that analysis of the determination of drive state by deprivation schedule and narrowly-defined FD is a problem of physiological psychology. More recently, in the same vein, Jensen (59) has found that time measures of rats' behavior in a T-maze varied both with FD (1, 5, and 22 hr.) and with daily food ration over the preceding seven days. There was no interaction between FD and daily ration. Neither variable affected the percentage of correct turns in the maze.

*Drive and reward value.*—A very interesting experiment by Olds (89) suggested that there exist in the brain localized hunger-reward and sexual-reward systems distinct from each other. The evidence came from an investigation of the effects of FD and of injections of testosterone propionate in castrated male rats upon intracranial self-stimulation. In general, there was a negative correlation between these effects over a range of electrode placements in hypothalamic and telencephalic structures; i.e., in a placement for which FD increased self-stimulation, androgen tended to decrease it, and conversely. Zero correlation would have been sufficient for Olds' suggestion; an additional hypothesis is required by the fact that the relation is negative.

Campbell (27) found that preference thresholds for sucrose solutions in two-bottle tests with rats were lower at all levels, both absolutely and relatively, for Ss under FD than for satiated Ss. Similarly, preference thresholds between water and sucrose were materially lower for hungry than for satiated animals. Curves of  $DI/I$  plotted against log molar concentration for both hungry and satiated groups were U-shaped and resembled human psychophysical functions.

Comparing rats deprived of or satiated upon food or sucrose in discrimination learning to food or sucrose incentives, Schulz & Lawrence (99) found that, in general, learning occurred when the reward was appropriate to the deprivation and not otherwise; although some Ss learned with food reward even though they were satiated for food. Neither additive nor interactive effects between the two deprivations were significant.

The relation between deprivation and taste sensitivity is evidently not a closed issue, as one might have been led to think by the work of Pfaffmann & Bare (92) and Meyer (81). Yensen (126, 127, 128), employing the method of limits with human Ss, found the absolute thresholds for sweet, sour, salt, and bitter to decrease for an hour or so after a meal and then to increase after three or four hours. The extent of the decrease was directly related to the caloric value of the meal. Further, loss of body salt decreased

the salt threshold, and loss of water increased it, with no changes in the other three thresholds. While Yensen believes that these changes are part of a biological mechanism for controlling intake of required substances, the reviewer has never been able to understand why, for example, increased salt sensitivity should lead to increased, rather than decreased, ingestion of salt. Indeed, Yensen found that reported hunger during fasting was inversely related to taste sensitivity. Preference tests for simple and complex taste-substances, associated with sensitivity measures, are much to be desired.

Wayner & Emmers (118) claimed that "need state" and "motivation" were independent in their results obtained from rats in a runway. Using water intake as an index of need state and number of runs to WR in the runway as an index of motivation, the former was found not to vary with time following subcutaneous injection of NaCl while the latter increased during the 4-hr. period. The results may be of interest even if these identifications are rejected.

*Irrelevant drive.*—The notion of irrelevant drive has taken a central role in discussions of drive as an energizer [Bolles (14); Estes (41); Spence (107)]. One difficulty has been that of finding a source of irrelevant drive that would not produce responses incompatible with the indicator response. After electric shock and loud noise failed by this criterion, Webb & Goodman (119) induced irrelevant escape motivation by flooding the box in which rats had previously been trained to press one of two bars for food reward (FR) under FD. Ss that were satiated for food pressed both bars more often under the flooding condition than in its absence. A strong bias toward the previously correct bar also appeared only under flooding.

Successful use of shock as a source of irrelevant drive in a quite different situation was, however, reported by Levine, Staats & Frommer (70). Their rats were required to swim to escape from a water maze; a group that was subjected to shock before the first of five trials per day swam faster than a nonshocked control group. No difference in errors was found. Levine *et al.* think that Dinsmoor (37) may have failed to find significant effects from adding irrelevant FD to electric shock because his Ss were already at nearly maximal motivation.

With rats reinforced for bar pressing only by a weak light, Clayton (30), Davis (36), and Segal (102) found that response rate increased with FD.

It has been possible to argue plausibly that results from the use of FD and WD are contaminated as evidence for irrelevant drive as an activator by common elements in the stimulus cues associated with hunger and thirst [e.g., Estes (41)]. The five studies just reported have enough variety to provide a challenge for this type of argument.

*Stimulus generalization.*—Two experiments on visual stimulus generalization as a function of drive in pigeons have appeared, both of which

used situations resembling that of Brush *et al.* (25). Jenkins, Pascal & Walker (58) found that gradients of generalization to illuminated spots larger and smaller than the CS were steeper for birds under low drive (80 per cent of normal body weight) than under high drive (70 per cent of normal body weight). Response rates were considerably higher at high than at low drive. Within the drive groups Ss with relatively high rates to the CS gave the flatter curves. The authors interpreted this finding as meaning that drive increase flattens the gradients by increasing response strength. This seems, however, to require the doubtful assumption that drive was constant within groups. Thomas & King (116) used a CS of 550 m $\mu$  and generalization stimuli ranging from 490 to 610 m $\mu$  with birds at 60, 70, 80, and 90 per cent of normal body weight. Both rate of pecking and amount of generalization increased monotonically with increased drive (i.e., decreased body weight). When gradients of relative generalization were formed by taking number of responses to each stimulus as a percentage of total number of responses, the 80 per cent group had the steepest slope and the 60 per cent group the flattest. Further experiments suggested that this non-monotonicity was due to drive during testing rather than to drive-stimulus generalization. The slopes of gradients of high and low responding Ss, three from each drive group, did not differ significantly; the authors suggested that this discrepancy with the results of Jenkins, Pascal & Walker may have been due to the difference in the stimuli used in the two experiments.

In a very different setting, Gewirtz (48) studied the generalization of children's preferences over a series of formboard puzzles, the shapes of which were varied in order to create a similarity dimension. Among Ss who gave preference-gradients along this dimension, those who had received negative reinforcement (failure, reproof, and withholding of a trinket) exhibited an avoidance gradient; i.e., the more similar the test puzzle to the training puzzle, the smaller the proportion of preferences for it. In the case of positive reinforcement (success, approval, and receipt of a trinket), some Ss gave approach gradients and others, avoidance gradients. These were interpreted as being, in fact, approach gradients in both instances—approach toward easy success in the former case and toward solving a difficult problem (achievement motivation) in the latter. In this experiment, as in many others using human Ss, it is not easy to distinguish between effects of drive and those of incentive.

*Generality of deprivation effects.*—Arguing that the effects of deprivation are principally associative, Bolles (16) presented results from 51 rats tested in six situations (open field, consummatory, elevated runway, alley, home cage, and stabilimeter) from which 20 measures of performance were taken. Food and water schedules were arranged in order to produce low and high hunger and low and high thirst. As in Anderson's (2) somewhat similar study of more than 20 years ago, the outcomes were largely negative.



Most of the performance measures were unaffected by either kind or amount of deprivation, and individual differences were "... clearly a function of situational factors, having little generality across deprivation conditions unless the situation was held constant" (p. 585). Nevertheless, the reviewer is inclined to take a less pessimistic view of the drive concept. An enormous variety of performance measures have, in fact, been found to vary regularly with deprivation. In Bolles's experiment, the four drive conditions were induced day by day in various cyclical orders; taking into account what has been said above about the effects of over-all schedule, it is hard to tell what the drive condition on any particular day would be. Recognizing this, Bolles argued that in spite of the failure of many performance measures to show consistent effects of deprivation, "The Ss always knew what and how much to consume" (p. 583). The data presented on this point are not, however, very striking. It is also possible that the absence of main effects of motivational conditions is due to lack of sensitivity of the design: the error term had to include various confounded interactions, including at least one—Ss by Motivation Conditions—that might reasonably be expected to be large.

#### DRIVE, LEARNING, AND PERFORMANCE

*Discrimination learning.*—Spence, Goodrich & Ross (108) have made an important contribution to our understanding of the effects of drive upon reduction of errors in differential learning. They trained rats in black-white discrimination in a box permitting E to give either a forced run into one alley or a choice between two alleys. In their first experiment the numbers of runs to white and black were equalized by forced trials; in this case, although animals under 40-hr. FD ran faster in the alleys than those under 3-hr. FD, the two groups did not differ in relative frequency of correct choices when choices were allowed. When, however, a second experiment was performed in which blocks of two forced trials and one choice trial were employed with the number of runs in the positive color twice that in the negative color, the 40-hr. Ss gave a higher relative frequency of correct runs than the 3-hr. Ss on the free runs; the high-drive Ss ran faster in the positive alley, but slower in the negative alley, than the low-drive Ss. The interpretation is that drive differences will be reflected in differential reduction of errors only in the presence of unequal amounts of experience with the alternatives.

No effects of differential FD of 1, 5, and 20 hr. were found by Miles (83) in brightness discrimination by squirrel monkeys, although significant differences occurred among individuals and between problems at two levels of difficulty. These results agreed with those of Meyer (82).

*Discrimination reversal.*—Since both intermittent reinforcement during acquisition and high drive during extinction tend to retard extinction, Kendler & Lachman (61) predicted that these conditions would retard re-

versal learning during that phase in which the now incorrect habit was dominant. Results from rats in a Y-maze (three identical arms), in which light vs. dark alleys were to be discriminated under 3- or 45-hr. FD during reversal learning, confirmed the hypothesis. The authors pointed out several difficulties of such experiments, however, and concluded that a single experiment can hardly settle the questions since "... the sharp theoretical distinction between drive and habit is not mirrored in experimental manipulations. The variation of a single independent variable, such as deprivation period, can influence both drive and habit" (p. 590).

Bolles (15) repeated and confirmed an earlier study by Wickens, Hall & Reid (121) on the effects of maintaining the same drive throughout position-reversal learning in rats as against shifting between FD and WD with each reversal. The latter procedure required fewer trials and produced fewer errors. Bolles attributed this to the greater "variability" of Ss under FD than under WD, which would lead the former to make a correct reversal response sooner but make more errors afterward. [Cf. Petrinovich & Bolles (91).]

Bruner *et al.* (23) argued that high drive during learning of a maze requiring response alternation should be adverse to "recoding" by the principle of alternation, so that reversal of the response sequence would be more difficult than under low drive. Their results for rats under 12- or 36-hr. FD were such a complex function of drive level during initial learning, drive during reversal, and amount of overlearning that no unconditional answer to the initial question could be given. One hopes for further work on this interesting question.

Hurwitz & Rowell (54) varied escape motivation in rats in a brightness-discrimination water maze by detaining them 10 or 40 sec. in the starting box. No significant differences in time or errors appeared in original learning, but when the cues were reversed and all Ss were detained 25 sec. (a shift downward for one group and upward for the other), the original 10-sec. group made significantly more errors than the 40-sec. group. The patterns of detention behavior during original learning—vigorous swimming and clawing in the 10-sec. group and motionless waiting in the 40-sec. group—were interchanged during reversal learning, leading the authors to suppose that successive trials involved both an increase in habit strength and a decrease in activation.

*Drive and habit.*—Attempts to compare the adequacy of the formulas for excitatory potential offered by Hull (53), (the product of drive, incentive, and habit strength) and Spence (106) (habit strength times the sum of drive and incentive), continue. Seward, Shea & Elkind (103) measured the running speed of rats in a runway under 0- or 23-hr. FD with FR or no FR. A strong interaction between FD and FR was observed: speed increased with trials when FD and FR were positive, but showed no change with trials when either, or both, were zero. The authors suggested that

Spence's additive formula may be correct under the first condition and Hull's under the latter conditions, a supposition that the reviewer does not find much to his taste. Besch & Reynolds (8) obtained indecisive results on the question whether drive and habit strength interact.

In a comparative study of a sort that is, unfortunately, rare, Haralson (51) observed bar pressing for WR by rats and target striking for FR by cichlid fish (*Tilapia macrocephala*). In both species partial satiation at extinction reduced resistance to extinction as measured by number of responses and by time. Interestingly, partial satiation during the latter part of acquisition reduced resistance to extinction in both species, even though the groups under comparison had made equal numbers of responses during that part of acquisition, a finding that may be taken as evidence for an effect of drive during acquisition upon learning as against performance.

*Classical conditioning.*—Spence has put forward the theory that in average classical conditioning the UCS functions as a reinforcer and as a means of arousing an emotional state that has the energizing properties of drive. In an admirable series of experiments, he and his collaborators have tested and confirmed this theory. The work has been done with eyelid conditioning in human Ss, using an air puff as UCS and the increase in brightness of a weak light as CS. To disentangle the two roles of the air puff, he invented the ingenious strategy of employing one strength of puff paired with the CS, and another strength of puff not so paired, the two being presented in an irregular order. It was thus possible to vary the strength of reinforcement with drive held constant over the series, or to vary drive over the series while holding strength of reinforcement constant. Complications arose from the presence of differential magnitude of response and adaptation to the air puff in the different drive groups, but, when these were allowed for, the evidence seems clear that the per cent of CRs is affected by the manipulations of the air puff just as would be expected from variation of drive and reinforcement, always, of course, assuming the theory correct [Spence, Haggard & Ross (109, 110); Trapold & Spence (117)]. See also Ross & Hunter (94).

For the same kind of conditioning, Runquist & Spence (95) obtained evidence that the air puff arouses the emotional responses and has the drive properties required by Spence's theory. Measures of pulse rate, skin conductance, and muscle action potentials (MAP) from the neck were used. The fact that the acquisition curves of conditioning diverged for groups of Ss differentiated by their MAP was taken to support the notion that drive and habit are related multiplicatively; it should be noted, however, that the curves do not appear to have reached asymptotic levels. The authors relate their conception of the motivating role of emotional responses to the physiological psychologist's notion of activation or arousal, mentioned above.

In connection with this it may be mentioned that Schnore (97) reported that differential instructions produced reliably different levels of arousal in

human Ss, as judged from nine physiological measures; but these differences were seen in individual patterns of response and would not appear if the measures were averaged over the Ss.

*Shock avoidance and escape.*—Boren, Sidman & Herrnstein (17) studied escape and avoidance behavior in rats as a function of shock intensities ranging from 0.1 to 3.7 m.amp. Pressing a bar turned off shock or postponed it for 20 sec. Rates of avoidance responding and stable levels of extinction responding increased with shock, whereas latencies of escape responses decreased; each of the functions appeared to approach an asymptote, although some Ss seemed to produce escape responses as rapidly as possible as soon as shock was strong enough to support escape behavior. The conditions under which such functions go through a maximum or approach an asymptote are not clear from earlier work [see, e.g., Kimble (62) and Brush (24)], but Boren *et al.* cite the work of Barry & Harrison (7) as suggesting that the use of intermittent reinforcement favors the occurrence of a maximum.

The acquisition of fear by rats as a function of shock intensity (0.14, 0.2, or 0.4 m.amp.) and CS intensity (6-ft.-c. light and 30-db buzzer or 18-ft.-c. light and 50-db buzzer) was studied by Mathers (78). After appropriate training, rats trained under high shock extinguished fastest and those trained to the intermediate shock extinguished slowest. The stronger of the two light-buzzer combinations produced the slower extinction. It should be noted that the CS was present during extinction and that the rats presumably had to learn to use the wheel to turn off the CS during this period; wheel turning had previously been reinforced only by shock termination.

Weiss & Laties (120) put rats in a situation in which by pressing a bar they could reduce electric shock that ranged from 0.07 to 0.65 m.amp. in 25 approximately equal steps. The shock increased by one step at a fixed time interval of 1, 2, 5, or 10 sec., and a fixed number of bar presses was required to produce a shock decrement. It was found that the percentage of time the rats took each shock level decreased with shock intensity and that the curves were flatter, the shorter the interval between shock increments.

*Complex processes in human Ss.*—A mathematical model for the effects of drive in complex learning was tested by Birch (11). According to the model, increased drive produces improved performance when the correct response is dominant but no improvement, or a decrement, when competing responses are nearly equal in strength to the correct response. Results interpreted as generally in agreement with the model were obtained from paired-associate learning under differential instructions as to competition and a money prize.

FD as a variable in human verbal learning was investigated by Lerner, Singer & Triandis (68). They supposed that hungry Ss would have a set toward food words and that these words would therefore be better differ-

entiated than nonfood words; if so, food words should be better cues than nonfood words for words following them in a list, according to Gibson's (49) theory. Ss tested at different times after a meal gave results agreeing with this prediction, although the groups did not differ significantly in learning food words as responses or in learning nonfood words. Perhaps it would be well to test separately the two assumptions, first, that hunger produces the required set and, second, that such a set (however aroused) has the hypothesized effects.

In a motor conflict task, Andreas (3) found that threat of increased shock punishment for incorrect responses led to longer response times in conflict trials, but differential shock punishment on nonconflict training trials did not produce significant differences.

Airmen in basic training improved their performance on a stick-and-rudder task under verbally induced increase in motivation only if they fell above the median in performance before introduction of the motivational variable [Fleishman (43)].

#### INCENTIVE MAGNITUDE AND ANIMAL BEHAVIOR

*Incentive and learning.*—Only material not covered by Pubols' (93) review will be discussed; however, this includes several studies of more than usual interest.

Leventhal *et al.* (69) had the interesting idea, which deserves development in several quite different directions, of comparing rats' frequencies of choices between a constant reward (one unit) and a variable reward (equally often two units and none). So, for example, 0.25 gm. of food was always on one side of an E-shaped maze, while the other side had 0.50 gm. for half the trials and none for the other half. This principle was employed in four conditions: FD and large FR, FD and small FR, WD and large WR, WD and small WR. With the smaller rewards, a strong tendency to choose the 50 per cent, 2-unit side developed over 112 to 160 trials, but no such difference occurred with the larger rewards. The authors suggest that the former result may mean that the small rewards are below optimum for consumption and digestion, leading, in this case, to a preference for two units over one. The problem should interest mathematical modelers and decision theorists, as well as psychologists concerned with partial reinforcement.

Wilson (124) studied choices made by rhesus monkeys between pairs of transparent boxes containing 1, 2, 3, 4, or 5 glucose pellets. The relative frequency of choices of the larger number of pellets increased with practice but, judging from the one learning curve shown, did not reach asymptote even over the course of 300 trials. Response latencies were longer as the difference in number of pellets between one box and the other decreased and, also, as the greater number in the pair decreased. In an amusing variation, the boxes were arranged so that the pellets visible to S were the dis-

criminanda for those that were actually obtained when S opened the boxes. If S chose the box with the larger number of pellets visible, he received four pellets; if he chose the smaller, he received none. Correct choices were made in about 90 per cent of the choices under these conditions. The procedure begs for a reversal in which the larger reward would be gained by choice of the smaller number of "cue" pellets.

The two studies just cited exemplify the "differential" method [Lawson (65); Pubols (93)] since each S was exposed to the various incentive magnitudes that were being compared. (In the experiment of Leventhal *et al.* one of the rewards was probabilistic between two different values.) The differential method has, on the whole, been more successful in producing reliable variations of response measures with variations of incentive than the "absolute" method, in which each S receives only one of the various amounts of reinforcement during the experiment. Lawson, Cross & Tambe (66) thought that the appearance of such differences would be assured by giving all Ss all amounts of incentive during pretraining in a different situation. They tested rats in a multiple Y-maze with a correction procedure after no prior experience, or experience with a small FR (118 mgm.), or a large FR (472 mgm.), or both large and small FR. To their surprise, errors and trials to a criterion varied appropriately with the incentive used in the Y-maze in all groups, and prior experience had no significant effect. Larger rewards resulted in faster running and more rapid improvement in running time.

Using the absolute method with rats in an H-maze, Bower & Trapold (22) found that terminal speed in a positive goal box was greater with eight pellets FR than with one pellet. Speed in a negative goal box, which never contained FR, was unrelated to the incentive received by S on positive trials, but speed in the main alley, which presented no cues as to which goal box or FR was present on a given trial, varied directly with amount of reward (on both rewarded and nonrewarded trials, of course). When nonrewarded trials were no longer given, speed increased strikingly; this seemed to be due to elimination of slowing down at the point at which S could "observe" cues to the incentive.

Furchtgott & Salzberg (46) found no difference in errors or trials to criterion of rats' running in a two-unit linear T-maze to sucrose concentrations of 4 and 16 per cent, but both of these groups were superior on both measures to a 2 per cent group, which, in turn, was superior to a water-incentive group. Running speeds increased with sucrose concentration.

After reviewing studies of the relation between acquired reward value and magnitude of primary reward, Pubols (93) concluded that the two are probably positively related, but that the relationship is more likely to appear with the differential than with the absolute method. One additional experiment on this problem has been reported. Stebbins (111), using the absolute method with Skinner boxes, found that the secondary reinforcing power of



light and the sound associated with the activation of a dipper that presented sucrose concentrations of 5, 8, 12.7, 20.1, 32, or 50 per cent increased with these concentrations up to 32 per cent, beyond which it appeared to be asymptotic. During the original discrimination training, response rates under light conditions increased with concentration up to 32 per cent and dropped sharply at 50 per cent; under dark conditions the rates were low and approximately the same for all groups. The results confirmed those obtained earlier by Butter & Thomas (26) insofar as the two experiments were similar.

A falling off of response rate or other measures of behavior at high concentrations of sweet substances has been observed so often as to be taken for granted. A further illustration was provided by Smith & Ross (105) who found that C57 black mice drank increasing quantities of sodium sucaryl as its concentration increased to 1.0 gm. per 100 ml. of water and that they drank decreasing amounts of greater concentrations. On the other hand, no such maximum was found by Stebbins, Mead & Martin (112) who employed sucrose concentrations of 5, 12.7, 32, and 50 per cent with each of two rats under a 2-min., fixed-interval reinforcement schedule; rather, the response rate increased sharply with concentration up to its highest value. These authors believed that the falling off at high concentrations was due to satiation rather than aversion and could be eliminated by the use of sufficiently small amounts of reinforcement (0.02 ml., in their own case).

Young, Falk & Kappauf (130) found little regularity in rats' latencies and running times to salt solutions of 0.1, 0.3, 0.9, 2.7, and 8.1 per cent concentration by weight. The 2.7 per cent solution was approached most rapidly, although preference tests indicate that 0.9 is preferred to 2.7.

The problem of the relation between the absolute and differential methods seems to need thorough conceptual analysis. It is sometimes asserted that animals must discriminate between two magnitudes of incentive if they are to respond differently to them. If this means that failure of the absolute method to produce differential behavior with different incentive magnitudes is to be accounted for by lack of discrimination among these magnitudes, the rather odd implication would seem to be that, in the absence of such discrimination, all magnitudes of incentive greater than some threshold value are equally effective (since learning and extinction do occur). One hesitates to believe this.

*Activity and alternation.*—Hall (50) compared activity-wheel measures of Ss receiving food immediately after their last opportunity to run during a deprivation period with those of Ss locked out of the wheels during the same period. The daily activity of the former group rose above that of the latter group and continued so over the remainder of the 18-day period. Hall believed that this indicated reinforcement of activity by food reward, but admitted that he could not assess the effects of the extinction that ought to occur as a result of unrewarded running.

Avoidance of repetition of responses ("alternation") has often been observed in choice experiments. Fowler, Blond & Dember (44) found less alternation by rats in a T-maze with an incentive of eight pellets than with one of two pellets and less also when incentive was contingent upon choice than when it was not.

Fowler, Fowler & Dember (45) studied the effect of varying motivation by electric shock, and of varying reward by escape from shock, upon rats' tendencies to repeat on Trial 2 the response made on Trial 1 in a T-maze. Shocks were 20, 40, 60, 80, or 100 v. through 0.3 megohm resistance; reward was absence of shock in the goal box. The amount of such repetition increased with shock and with shock reduction; evidence was offered that it was the amount of reward, rather than the motivation, that was effective. The authors believed that the rule, in studies of this problem, is that small amounts of reward fail to increase repetition (or, to decrease "alternation") but that strong reward does so.

*Learning vs. performance.*—Work continues on this difficult problem. Collier & Marx (32) trained rats in bar pressing with incentives of 4, 11.3, or 32 per cent sucrose solutions, then tested all of them over a period of 10 days on 1-min. periodic reinforcement with an incentive of 11.3 per cent sucrose. Number of reinforcements received during training did not vary significantly. During the test period, the Ss originally trained with 4 per cent consistently gave the greatest number of responses, and those trained with 32 per cent the smallest. If these are contrast effects, they are evidently quite stable and durable. When extinction was induced, the Ss originally trained on 4 per cent extinguished fastest and those on 32 per cent slowest. The authors were inclined to interpret the results, in the language of psychophysics, as effects of anchoring or adaptation level, although failure of the curves to converge during testing was difficult to account for in this way. See also Bevan & Adamson (9), referred to later.

Results obtained by Armus (4) raised similar questions. He trained one group of rats in a runway to one 45-mgm. pellet and another to 10 such pellets. The latter group was superior to the former in both starting and running times. In extinction, the starting times did not differ significantly, but, by the criterion of running time, the 10-pellet group extinguished faster than the one-pellet group.

Pubols (93) concluded that "Asymptotic performance is an increasing function of incentive magnitude," and that "Magnitude of reward affects resistance to extinction indirectly through differences in terminal level of rewarded performance" (p. 111). The two studies just reviewed point to some of the limitations that contrast effects, whatever their basis, impose upon these conclusions.

Learning and resistance to extinction of rats in alleys under variation of food reward were studied by Lawson *et al.* (67). During training the animals received either always four pellets, or always one pellet, or half the

time four and half the time one. It was reported that the reward conditions did not affect the number of trials required to attain a stable level of performance. In extinction, latencies and total times increased most rapidly for the four-pellet group and least rapidly for the half-four, half-one group.

Bower, Fowler and Trapold (21) ran rats in an electrified start box and alley (250 v. through 0.25 megohm resistance) to a goal box with 50, 150, or 200 v. for 30 trials. Separate groups were used, as in the absolute method. The three groups reached different stable levels of running speed, the 50-v. group being fastest and the 200-v. group showing little, if any, learning. When subgroups of the 50- and 200-v. groups were then shifted to 150 and 200, and 150 and 50 v., respectively, in the goal box, the curves in each case approached closely the appropriate asymptote—a beautiful result. But why did contrast effects not appear?

#### INCENTIVES IN HUMAN BEHAVIOR

*Binary prediction and similar choice situations.*—With college students who selected on each trial one of a large number of patterns of buttons to be pressed on a machine and who were rewarded 0, 1, 3, 6, or 9 times in nine acquisition trials, Lewis & Duncan (72) found a nearly linear increase in mean log plays to extinction (voluntarily quitting the game) with 1, 10, 25, and 50 cents reward on the rewarded plays. Ratings of expectation of reward showed no main effects attributable to amount of reward in either acquisition or extinction, although some significant interactions appeared. Earlier experiments found fairly consistently that, with probability constant, Ss are more likely to predict the occurrence of an event if it is desirable than if it is undesirable [Marks (77); Irwin (55); Jessor & Readio (60); Crandall, Solomon & Kellaway (34)]. Crandall, Solomon & Kellaway (35), in a procedure more closely resembling that of Lewis & Duncan, found that the relative frequency of predictions of a desirable event increased more rapidly and extinguished more slowly than those of an undesirable event. Perhaps the discrepancy between Lewis & Duncan's results and the earlier findings is a reflection of the fact that Lewis & Duncan used only positive rewards, whereas negative rewards (losses) were used in all of the other cases cited. Common observation would suggest that there ought to be conditions under which "pessimistic" predictions would be made, but this has yet to be reported except for occasional instances in the literature on level of aspiration [e.g., Sears (100)].

In a binary choice situation, Edwards (38) varied both the amount of monetary reward and the probability of reward of the alternatives. He reported (p. 181) that, "It is possible to compensate for changes in probability by reverse changes in amount of reward." Siegel & Goldstein (104) had Ss predict which of two lights would go on under conditions of No Payoff, Reward (5 cents per correct prediction), and Risk (5 cents won for correct prediction and 5 cents lost for incorrect prediction). The rela-

tive frequency of predictions of the more frequent event increased over the three conditions in the order mentioned. Irwin & Smith (56) required Ss to decide whether the mean of the numbers on cards in shuffled packs was greater than or less than zero, the cards being drawn one at a time. The greater the money prize (\$1.00 vs. \$.50) for a correct decision and the less the cost per card (5 cents vs. 10 cents), the more cards the Ss drew in order to make a decision.

Whether with rats in T-mazes or human beings in binary choice situations, it seems to the reviewer that evidence for the inadequacy of present forms of trial-by-trial analysis mounts. This is reflected in the fact that conceptions resembling "hypotheses" are being introduced more and more frequently. Galanter & Smith (47), looking at binary prediction by college students as a "simple thought problem," have contributed to this trend in two ways. First, they showed that increasing the "cost" of frequency-matching behavior by introducing trials in which predictions had to hold for five consecutive trials, instead of the usual one, produced increases in the frequency of predictions of the more frequent event on these special trials, but predictions on regular trials were essentially unaffected. They also found that a payoff function having a maximum money reward for 100 per cent correct predictions led Ss to predict the more frequent event (in a 75-25 schedule) with a relative frequency of .90, whereas this relative frequency was approximately the matching value (.75) when the payoff function was maximal for predictions correct 70 or more per cent of the time.

In spite of the studies just reviewed, it is clear that there are conditions in which increasing the payoff for correct responses may decrease, rather than increase, the relative frequency of predictions of the more rewarding event. In one experiment, five-year old children were rewarded 100, 66, or 33 per cent of the time for pressing one particular button out of three, the other two buttons being unrewarded. Marbles and plastic trinkets were the incentives, the latter being preferred. While the preferred incentive led eventually to a slightly higher frequency of correct responses under 100 per cent reinforcement, it produced materially lower frequencies of correct responses under 66 and 33 per cent reinforcement [Stevenson & Weir (115)]. The authors believed that the performance under the latter conditions did not indicate how well the Ss knew which button was reinforcing, but that, rather, "... it indicates the degree to which S will accept the percentage of reinforcement provided by the reinforcing stimulus" (p. 407). Woods (125) made the same point. He required college students to press one of two switches to turn on a red light and turn off a noise of 70, 80, or 95 db, offering one of three pairs of probabilities of reward, 98-58, 87-47, or 53-13. The lowest and highest levels of noise (and, of course, of noise reduction, with which noise was confounded) tended to give higher rates of acquisition than the intermediate level. The author suggested that stronger

motivation may lead S to try out more hypotheses about the series and thus cause a relatively large number of choices of the less rewarding alternative. Some accessory principle would be needed, however, to account for the high frequency of choices of the more rewarded side under the highest level of motivation in this experiment.

An experiment was arranged by Lanzetta & Kanareff (64) in such a way that an S could achieve 80 per cent "correct" pitch-discrimination judgments by imitating or opposing, as the case might be, judgments of a fictitious partner who judged first. Introduction of a money payoff overcame the attenuation of imitative and opposing responses that was otherwise produced by instructions toward independence of judgment; but, contrary to the authors' expectations, the money payoff also attenuated the tendencies toward imitation and opposition under neutral instructions. It is easy to suppose, in conformity with the principle mentioned above, that the payoff made it worth while for Ss to attempt a variety of hypotheses and thus resulted in smaller degrees of association with the "partner's" judgments.

*Reaction time, errors, and other performance measures.*—Botwinick, Brinley & Robbin (18, 19, 20) obtained significant improvement in auditory reaction times by introducing electric shock punishment for slow responses. The effect occurred in both young and old Ss (18 to 37 and 65 to 81 years, respectively).

Murphy (86) compared the effects of the presence and absence of threat of shock upon performance. In his case, the Ss had to call off the rings that were marked on a series of range-ring patterns. Threat of shock (no actual shocks were administered) resulted in increased time measures but decreased frequency of errors, an outcome that is hardly surprising in view of the fact that shock had been made contingent upon errors but not upon time.

As a result of shifting from control conditions, in which the Ss were merely told to work hard, to conditions in which they were given encouragement and sympathy, O'Connor & Claridge (88) found a pronounced positive contrast effect ("elation") in male imbeciles ranging from 18 to 45 years of age, working on a task of putting pins in holes. The effect lasted without diminution over an eight-day period with one hour of work per day. A group shifted in the opposite direction showed little change.

Bevan & Adamson (9) argued that the effects of amount of reward, shifts of incentive, etc., ought to be predictable from such considerations as enter into the scaling of psychophysical stimuli and adaptation-level theory. In a series of experiments with maze performance of human Ss, they were able, by varying intensity of shock punishment for errors, to obtain outcomes that varied much as expected on the basis of prior category judgments of shock intensities. Effects resembling contrast and averaging of the shock stimuli over a series were discovered. Schneider & Baker (96) obtained fairly reliable ratings of the unpleasantness of shock to the index

and third fingers, using 20 levels of shock ranging from 0.2 to 3.0 m.amp., and offered their technique as a means of inducing graded motivation in human Ss. They did not, however, report results from application of the technique.

*Time estimation.*—Two reports on estimations of the length of a period of time as a function of motivational variables will be mentioned here, although it is not easy to decide the extent to which incentive factors, as distinguished from "drive" factors, were critical. Meade's (80) Ss worked blindfolded on a stylus maze for 6 min. and were then asked to judge how long they had worked. The "high motivation" Ss were told that, if they performed well enough, they would escape the necessity of about two hours or boring tasks, while the "low motivation" Ss were told that they would go on with these same tasks when the maze task was finished. The Ss were also divided factorially according to whether they were led to believe that they were making slow or rapid progress during the task and whether, at the time of interruption, they had done 20, 50, or 80 per cent of the task. Ss with low motivation gave relatively high estimates of the time and were not affected by the variables of rate of progress or distance from the goal; but, in Ss with high motivation, estimates of duration were inversely related to the rate of progress and directly related to the distance from the goal at the point of interruption. Schönbach (98) had college girls estimate the length of a 13-min. period of delay before a "food-tasting" procedure. Some had not eaten on that day and were tested at 3:00 or 4:00 p.m., while the others ate normally and were tested at 1:00 or 2:00 p.m. The delay period was filled with "relevant" ratings of dishes described in a cookbook or with "irrelevant" ratings of fashions. The results were interpreted as indicating that the greater the force on a person toward a goal, the greater his estimate of time spent in a barrier situation. All of these results, together with those of earlier work on the same problem [e.g., Hindle (52)], seem to agree with the hypothesis that the more S desires that an interval of time pass rapidly, the longer it will appear to be.

#### CONCLUDING REMARKS

Judged by productivity relative to other fields, lawfulness of the phenomena achieved under favorable conditions, and increasing subtlety of analysis (both experimental and theoretical), the study of quantitative variation of motivational processes is thriving. Complacency is, of course, not in order; it is clear that we are only beginning to identify and understand the problems. Moreover, there are persistent sources of dissatisfaction with the ongoing effort that must disturb anyone who reviews a sufficiently large segment of the work. Some of these need to be pointed out at the expense of repeating complaints frequently heard before and even though they are seldom peculiar to this field.

(a) Invention of new experimental conditions—apparatus, deprivation



schedules, response measures, etc.—where neither theory nor empirical problem demands them, could well be spared in favor of greater standardization. The use of previously published methods should receive a high mark from editors and general readers.

(b) Parametric experiments should supplant studies designed to determine whether a variable does or does not have an effect.

(c) It is known that central tendencies of groups are sometimes very misleading with respect to the behavior of individuals. This knowledge should be used more often.

(d) The development of mathematical psychology has shown how strong (and consequently, how restrictive) the most innocent-looking theoretical assumption can be, yet assumptions of great strength continue to be set forward without much apparent concern for their implications. A special case of this is the almost reckless use of transformations of variables; when theory is involved, convenience of graphic presentation surely ought not to be decisive for such choices [cf. Estes (42)]. Proper distinctions between empirical and definitional questions are fundamental, however difficult they may be.

(e) Studies of motivation exhibit an interesting duality, of which, perhaps, more use could be made. One can ask, what is the effect of variation in drive upon activity, exploration, measures of speed or of errors in simple and complex learning, learning vs. performance, acquisition of secondary reinforcing value, etc., etc.? Each of these questions can be asked also about incentive, but where it is possible to ask parallel questions, it might be well to do so as a routine. The point was made above in the discussion of the work of Wike *et al.* (122, 123), and a pretty example is seen in the study of Pereboom & Crawford (90) following upon that of Cotton (33). Stebbins evidently had something of this sort in mind when, in the context of the relation of primary to secondary reinforcement, he commented:

Whereas changes in motivation [i.e., what we have referred to as "drive"] tend to bring about widespread changes in behavior irrespective of the stimulus situation, behavioral change as a function of variation in amount of reinforcement would appear to be limited to the specific stimulus condition under which the reinforcement prevails (111, p. 723).

The question whether such a generalization is valid exemplifies the issues that reflection upon the duality of motivational problems would confront.

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## MOTOR-SKILLS LEARNING<sup>1,2</sup>

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With everyone else, we think that task areas are largely a convenience, and it is thus not worth while to labor a definition of motor skills. Research in motor-skills learning is conducted for the same reason that any research is conducted. Learning is learning, regardless of Ss and apparatus, and we wish to make no point of motor-skills learning as opposed to verbal learning or conditioning. Nonetheless, we have tried to include only studies that are well within our crudely defined area of human motor-skills learning, however strong the temptation to follow a major variable through the single-unit T-maze and memory drum.

Among the complex parts of human learning, we distinguish very roughly and with deliberate naivete as follows—motor skills: the hand holds and moves some physical apparatus, pencil and toggle-switch usually excepted, unless S cannot make the required movements at the start of practice; verbal learning: the task emphasizes words to be memorized; and perceptual learning: it is neither of the above and the senses are the most obvious elements of the task. A simpler way to say about the same thing is to distinguish areas according to the relative importance of the hand, tongue, and eye. Also implicit in motor-skills learning is the emphasis on learning to make R, rather than to select R on cue, an interpretation that has severely limited our review of push-button research.

The danger of sorting research into convenient piles is that studies that land between piles may be overlooked. This is a problem not only in separating verbal from motor studies, but in separating human-factors engineering studies from skills learning. With both studying common performance (not learning) variables, the relationships between the two areas are too obvious to list; as a result, many fine publications at the interface will be neglected. An example is Krendel's (132) summary of performance experiments on man-powered devices. Spragg, Andreas, Green, Gerall, and many associates in the skills laboratory at Rochester have been steadily producing data on display-control relations and on the response properties of controls and displays (21, 22, 23, 101). A long line of careful work by Smith (112, 199,

<sup>1</sup> This review covers the years 1945 through 1959. Previous reviews overlooked skills learning, so our expanded coverage cites critical articles from 1945-1955, but gives greatest emphasis to the years after 1955 as reference sources. The writers wish to acknowledge the financial contribution of the Graduate Council on Research of Tulane University and the support of the National Science Foundation.

<sup>2</sup> The following abbreviations and symbols will be used: S (subject); E (experimenter); R (response); S-R (stimulus-response); KR (knowledge of results or feedback); I<sub>R</sub> (reactive inhibition);  $\mathbf{I}_R$  (conditioned inhibition).

200) at Wisconsin also lies somewhere near the boundaries, though clearly within the learning area. British contributors whose output appeared not as immediately relevant are Conrad, Crossman, Leonard, Mackworth, Poulton, and von Wright.

The human-factors engineering literature shows the fact that selecting apparatus is a major step in making appropriate tests of learning hypotheses. Apparatus provide variables with which Rs and their measures may vary—sometimes critically. The reader is especially directed to the work of Fitts and his associates (34, 170) and of Taylor and his associates (70) for illustrations of the relevance of instrumentation. Fitts *et al.* expect Wiley to publish by 1961 an overview of their detailed, systematic, and thorough-going engineering approach to the study of skilled motor performance (93, 94). Bibliographies and abstracts of articles relating to skilled performance now appear yearly (116).

Certainly, analyzing display and control variables is extremely sophisticated in the hands of some investigators studying behavior in complex tracking systems. There are excellent reports of the physical properties of control sticks (such as spring stiffness, damping, and mass) and of various spatial and temporal Rs to these (accuracy, rate, and acceleration) (32, 35, 210). These investigations show that E's task of matching cues and Rs may be a greater problem to him than the S's task of responding to these cues with the required Rs (33). Besides their work on the control, the Fitts group has followed through with a systematic analysis of the other major components of tracking systems. Only representative studies can be cited: target and response frequency (170), target and response amplitude (68, 110), noise in the target and cursor (67, 115), control-display spatial relationships (92). Taylor & Birmingham (211) deal particularly well with the dangers of inferring properties of men from analyzing outputs of the products of apparatus  $\times$  men. In general, we find considerable and growing awareness of learning vs. performance differences in design and scoring (67, 68), and an increasing dependence upon training variables (98, 100).

#### APPARATUS

Extensive programs on motor-skills learning began after World War II. There were two direct causes. One was Hull's book in 1943, and especially his constructs of  $I_R$  and  $gI_R$ . The other cause was the familiarity with apparatus engendered by developing a battery of hardware to select aircrews for the United States Air Force. The early 1950s made thrifty use of this equipment—discrimination reaction time, complex co-ordination, two-hand co-ordination tests, gunnery trainers, and most of the other School of Aviation Medicine devices described in Melton (150). Though the war had ended with no important research on motor learning, service-sponsored post-war research more than made up for this. In 1949, for example, the Human Resources Research Center (later, Air Force Personnel and Training Re-

search Center) opened and supported an extraordinary, active program of in-service and contract research that in its first five years set the field ahead by more than 20. A good slice of the program and unprecedented amounts of money were channeled to the learning aspects of inherited hardware (incidentally, developed by apparatus-oriented Es with interests in conditioning). Melton, Brown, Buel, Fitts, Gagne, and Brogden, among others, were figures important in both pre- and postwar phases. The Aero Medical Laboratory, under Grether, also sponsored an active program of research on skills.

For a summary of the current picture, Table 1 lists the devices upon which most systematic research is now pursued; all but three of the items

TABLE 1  
MOTOR-SKILLS DEVICES IN COMMON USE, 1950 TO 1960

Apparatus	Major User	Apparatus	Major User
Mathometer	Noble (160)	Linear Pursuit	Brogden (215)
Star Discrimeter	Duncan (82)	Rotary Pursuit	Ammons (14)
	Lewis (140)	Electronic Tracking	Fitts <i>et al.</i>
Pressure Stick	Bahrack (169)	Apparatus; Res.	(67, 221)
Knobs, Levers, and Cranks	Bilodeau (59, 63)	Simulator	
Complex Co-ordi- nators	Lewis, Shephard (141, 194)	Electronic Tracking Apparatus	Garvey & Taylor (100)
		Gunnery and Flight Simulators	Many

were developed in the last decade. Developers, aficionados, or both are also shown. Other less tried devices are the null-point discontinuous pursuit meter (197), one- or two-hand tracking (56, 102, 133), pressure stick (207), ergograph (123), pencil-and-paper tracking (175), and the Michigan State University Serial Reactor (39). Thompson, Thompson & Dusek (214) have reviewed hundreds of tests of gross muscular co-ordination and list 255 references. These tests require standard gym equipment or no apparatus at all. Highly promising uses of closed circuit television for controlling visual feedback have been described by Smith *et al.* (200). As more flexible devices have developed, the once-ubiquitous rotary pursuit test in various forms, hereafter called rotor, is taking less and less journal space. In contrast with earlier equipment, the newer devices have the following dominant characteristics upon which their flexibility depends: program input is both rigidly specifiable and variable; the required or correct movements are variables; scoring is more analytic; and the control and display features do not necessarily covary.

Chances are better than four in five that the device to match the next

research proposal already exists. Most of the devices in Table 1 are multiple-purpose research tools, and the nature of their next employment cannot be guessed. There are lever devices, such as the Electronic Tracking Device used by Fitts, that employ continuous responding and continuous displays of action feedback. Devices such as Noble's Mathometer deliberately reduce the R component close to zero (microswitch controls). Displays can be eliminated or made as simple or complex as E requires. Bilodeau & Bilodeau use knobs and levers and no display in order to effect a useful simplification of task and apparatus. Feedbacks can be terminal or discontinuous (58, 167) or continuous (14, 221). More importantly, in the majority of devices of Table 1 the linkage between controls and feedback is not fixed; the biggest factor in the developing flexibility is that E can independently vary display and control features.

The significance of Table 1 is its implication that time both as independent (length of trial) and as dependent (time on target) variable is being dropped. Scoring has turned toward more informative measures of errors, position, rate, matches, pressures, time-sharing, information transmitted, etc., in line with rational interests in specific characteristics of skilled (vs. unskilled) performance and with generally felt doubts about time on target as a measure of skill. Moreover, more analytic approaches to rotor Rs have been amply demonstrated as possible, if difficult (13, 17, 27, 66); incidentally, these studies give little indication that time on target was an inappropriate measure for this device.

The change in scoring of Rs is promising, but it is on the side of the independent variable that the blessing is greatest. Ten years ago it seemed a real possibility that motor-skills data and theory would be permanently engulfed by distribution of temporal variables. Now, of the apparatus listed in Table 1, only the rotor is typically assigned to investigate such variables. Response as a function of time is being replaced with a variety of R-R and S-R relations, exploring R as functions of such diverse independent variables as pre-experimental S-R stereotypes, individual differences, task properties, and their interactions with trials.

#### ABILITIES, TRAINING, AND TASKS AS PREDICTORS OF SKILL

The standard approach to what is being learned with practice is the transfer of training design. Other approaches are being tried, however. The studies that follow have a kaleidoscopic quality, but all relate to predicting performance by means of factors of factor analysis, individual differences, or dimensions of tasks. These studies combine the differential-testing approach with the subject matter of learning, an influence not so much enjoyed by the other sides of the learning family. Our survey of abilities and psychomotor learning has a definite orientation, is highly selective, and avoids all questions of abilities and factors, or their breakdowns. As a source of references to psychomotor factors for the broad-minded, we sug-

gest Guilford's review (109). Reading his article raised questions (that the writers had better not answer), a couple of which we pass along. It is possible to make an intelligent *a priori* appraisal of the predictive value of psychomotor factors? What has the highly developed intelligence-testing effort contributed to the rest of the learning family, especially the verbal component?

One major group of experiments followed Reynolds' in the early 1950s, picking up some scattered threads and reintroducing correlational analysis to the question of what had been learned with practice. For earlier references see Reynolds (181) and Jones (122). Jones provides a thought-provoking review and integration of many of the correlational analyses of motor learning and proposes a new model that covers training as well as learning. Simplified, both Reynolds and Jones reduce the problem to accounting for the very striking and consistent intertrial pattern of coefficients obtained on learning tasks. That is, (*a*) correlations between the first trial and successive trials are progressively lower, whereas (*b*) correlations between adjacent trials are progressively larger (122, 181). Lewis, McAllister & Bechtoldt (138, 139) have demonstrated a very like pattern over different tasks on the same apparatus and interpret their findings in terms of common skill for all tasks. If the trends mean two factors (122), then  $I_R$ , and degree of massing, might relate to the factor that shows up as the more important the later the trial (180). That is, if  $I_R$  grows from Trial 1 on, so should its contribution to the variance, making  $I_R$  a good bet for some part of both the increasing consistency with practice and the decreasing communality between early and late performance. Thus, Reynolds (180) studied the correlations within and between devices as a function of distribution of practice on the device first administered. The present reviewers consider Reynolds' verification less striking than he did. In a later experiment (181), the breakdown in the correlation pattern with introduction of a major rest is very nice, but rest does more than reduce  $I_R$ . Nance's earlier study (156), in which he compared intertask correlations as affected by two variables, pacing and distribution, and, particularly, Kientzle's (124, 125) two sets of data are quite opposed to an  $I_R$  explanation of the pattern. Kientzle claimed essentially identical patterns and magnitudes of correlations with massed and distributed reversed-alphabet printing (124). Further, the correlation between pre- and postshift trials was completely unaffected by either distribution of practice or change in distribution of practice (125). These convincing data, of course, not only argue against  $I_R$  as the variable accounting for the pattern over learning trials, but provide a rather sturdy kick to  $I_R$  as an independent construct. The performance measures and the coefficients pose contradictions like those Lewis and his co-workers met in studying interference (136, 138, 139, 202). Absolute level of an S's performance is a function of distribution of practice; relative position is not [Kientzle (125)]. Riopelle (187), unfortunately with too few Ss and on



only two data points, and more recently Zeaman & Kaufman (226) have some positive evidence of distribution effects for magnitude (not trend) of correlations with Trial 1.

The distribution studies, rather than answering the first question, seem to us, at most, to raise a second: whether distribution of practice (or  $I_R$ ) affects intertrial coefficients. This may account for there being no present follow-up of distribution of practice and patterns of correlation. The major question still remains: "What learning of what" underlies increasing time on target and presumably, too, the correlation patterns? The next method, correlating external predictor tests with psychomotor performance at various stages of practice, was much more fruitful (181). In the same laboratory, Adams with correlational methods and Fleishman & Hempel with factor analysis joined the search, using School of Aviation Medicine apparatus tests as criterion tasks and other apparatus and paper-and-pencil tests as external predictors. To summarize the findings very grossly and inadequately: predictor paper-and-pencil tests from the Airman Classification Battery generally show decreasing correlation with performance on complex apparatus tests from early to late trials of practice (181), and the relative contribution of predictor tests of "speed" increases, whereas the relative contribution of "perceptual" tests decreases with increasing practice (2, 96, 181). The intertask measures thus account for the intratask pattern and suggest some change in the task requirements (or abilities) from early to late in learning. Reynolds' (181), as later Jones' (122), general interpretation was that the composition of performance became simpler as practice continued, with fewer contributing abilities involved in sophisticated than in naive performance. Jones, in demonstrating that the matrices fit the simplicial form he predicts, is alone in predicting the intratask pattern without external predictors and in advance of the data. Adams, Fleishman, Jones, and Reynolds all make the point that the progressive simplification could imply many things: the abilities sampled in the predictors (2, 96, 122), the decreasing advantage of pre-experimental experience as practice continues (7, 184), unique abilities for each task (96, 181), etc. Jones (122) makes a particularly good, if brief, case for predictability and against the nonsense to which uniqueness reduces. We are, thus, greatly in debt to Adams (2) for emphasizing the predictability of performance at any stage of practice, in a careful demonstration that: (a) predictor tests can give a bigger multiple correlation for final than for initial trials; (b) if practice is given on psychomotor predictor tests, performance on late predictor trials can predict final criterion performance as well as initial predictor trials can predict initial criterion performance; and (c) final level of criterion performance can be better predicted by extratask measures than by intratask measures (early performance on the criterion-learning task). Fleishman & Hempel (96) also show that late performance is as accountable for as early, and accounted for by as many factors. Adams' latest study (7)

presents the most coherent S-R learning account yet of how "abilities," pre-experimental experience, integration of components into total task, and experimental manipulations relate to the effects that learning has on inter- and intratask correlations.

There is a small, related body of studies giving dramatic emphasis to the meaning of intra- and extratask measures of abilities by demonstrating their effects on performance. Thus, Reynolds & Adams (184) presented very nice functions for rotor performance according to S's initial level of ability on the task, emphasizing that individual differences affect only the constants of the equations. Noble, Noble & Alcock (168), with the Selective Mathometer, relate the greater difficulty of a more complex task (10-R vs. 4-R chain) to greater factorial diversity, as well as verifying that externally predicted individual differences influence only constants. Adams (7) with discrimination reaction time and Cieutat & Noble (71) with two-hand co-ordination have essentially replicated the rotor findings, and Zeaman & Kaufman (226) relate initial ability to their measures of  $I_R$  and  $sI_R$  in reverse printing. Fleishman & Hempel (96) have some very pretty functions in which Ss were sorted and resorted according to various predictors. Their study gives a beautiful demonstration of the interactions of the predictors with criterion trials. Stratifying Ss into groups according to one predictor variable can lead to large initial differences in criterion performance and little or no final difference; another predictor might show no particular early difference and a sizable late difference, while a third might lead to fairly constant differences. These graphs spell out the performance implications of the correlational and factorial studies of various stages of learning, and they mean much for matching Ss on the basis of correlations between Trial 1 scores and external predictors.

The above studies employ lump-sum predictor scores. Criterion performance can be synthesized from more analytic individual task components. Such studies bear on the relationship between various components, how these relationships change with practice, and how the components are integrated either early or late in practice. Poulton's review (176) emphasizes the analysis of the underlying pattern of motion, as does a study by Simon & Simon (198). One of the present writers (54) offers a surprisingly accurate model making use of part-task probabilities for synthesizing the criterion time on target. Rubin & Smith (189) examine the correlation between components over four days of practice and find it low and unchanged from early to late trials. How far intercomponent relations can be taken or the lengths to which the devotees of skill may take the correlation coefficient is exemplified by Bilodeau (57). A single S was brought to peak proficiency on a four-part tracking task, and for this one S thousands of correlations between part and whole task probabilities were computed. The author was elated by the item-test analogy and then by beautiful patterns of accounted-for variance ranging from zero to 1.00. The

zero correlations obtained between individual parts, whereas increasing correlation was observed as a function of the regression of more and more parts upon the whole. The work on correlational and probability models has required a considerable physical plant. For simpler resources, however, though not strictly motor skills, Birch's combinational model can be used (64).

For successive, rather than simultaneous Rs, Noble and his colleagues have an ambitious program in selective learning. They have been specifying the effects of task and individual difference variables on the parameters of a rational learning equation (160). This program has produced basic work relating task difficulty to several aspects of task complexity (91, 159, 160, 162): variability in the S-R sequence, task length, and R availability.

#### THE FEEDBACK FAMILY

Studies of feedback or knowledge of results (KR) show it to be the strongest, most important variable controlling performance and learning. It has been shown repeatedly, as well as recently, that there is no improvement without KR, progressive improvement with it, and deterioration after its withdrawal (60). A number of studies (73) show that performance is seriously disrupted or made impossible by lags in feedback of even less than 1.0 sec. Furthermore, behavior is greatly enhanced with supplementary KR (201) and quickened KR (209). Other kinds of KR studies show that E can elicit  $R_a$ ,  $R_b$ , . . . or  $R_n$  at will, depending upon how he regulates the cueing feedback (51). No other independent variable offers the wide range of possibilities for getting man to repeat, or change his Rs immediately or slowly, by small or large amounts.

*History and comment.*—Laws consisting of statements relating R variations to feedback variations, as expressed by the general mathematical equation,  $R = f(KR)$ , have been sought by some. Though most Es would agree that R is some function of feedback, there is no agreement on the definition, never mind the function. Indeed, there is not even widespread agreement as to name; knowledge and feedback represent the core words, modified by other words such as results, performance, psychological, achievement, intrinsic, extrinsic, extra, supplementary, augmented, degraded, proprioceptive, incentive, social, etc. Feedback appears to be the more descriptive and harmless appellation. Whatever it is, and whatever it may be called, the work of the 1950s can be divided into three broad areas: (a) transformations, (b) temporal delay, and (c) supplements to the standard. A fourth area, frequency schedules, has hardly been touched. Before reviewing the data, we must note how various authors have dealt with or dodged the definitional problems.

Brown (69), Ammons (15), Annett & Kay (25), and Fitts *et al.* (94) take somewhat different positions on knowledge of results. All would include external events that depend upon what S has done and that are di-

rected back towards S. They disagree on whether S's knowledge or habits enter the definition. They also do not all face, recognize, or take a stand on what types of external stimulus feedback are admissible feedbacks. Apparently there is no present limit on what may be a legitimate feedback. After drawing a line, S might hear: "Too low," " $\frac{1}{4}$  unit low," " $\frac{1}{4}$  in. low," ".245 in. low," or "You moved  $2\frac{3}{4}$  in." Thus, numbers, signs, units, and goals are most certainly involved. E's evaluations of all these may be involved (and also even higher order codes, or orders of KR), i.e., "Here's a cookie," "Good," "In the top decile," "Ha ha," and "Unh ha," the only limitation being E's ingenuity.

Brown (69) says that giving KR is the process of providing the learner with information as to how good or how accurate his reactions are. In an earlier era, Seashore & Bavelas (193) argued that correct and incorrect conceptions of one's performance were included in KR. Ammons (15) refers to "knowledge of various kinds which the performer received about his performance." Annett & Kay (25) insist that it is S's perception of his KR that really counts. Taylor (209), Fitts *et al.* (94), and Bilodeau (53) use R error as if it were the essence of feedback and have so far managed to duck the question of the boundaries. A look at their work suggests that they are unhappy with R evaluation by either S or E as a basic element of feedback. They restrict feedback to observable, quantifiable events, as would Norbert Wiener. They believe that S's overt Rs to feedback are the objects of the inquiry and that the word "knowledge" in the phrase, knowledge of results, should not have the implication of a response to feedback.

When feedback is reduced to a number or represented on a display, it is an independent variable or stimulus coded and transformed in some way by E. It is either some function ( $f$ ) of R or of error in R. In either case, KR refers to external events on spatial and temporal co-ordinates. Feedback in simple pursuit tracking, for example, consists of two events: the locus of the control (limb, stylus, or cursor) and the locus of the goal stimulus; these events can be coded any number of ways, either within or between sensory modalities. For example, even the loci of stylus and target on the rotor could be represented numerically instead of visio-spatially, or, still visually, on a cathode ray tube at half of true scale (200). In the case of compensatory pursuit, feedback consists of one event, a representation of R error, that has multiple determinants. That is, the error displayed is some coded function of both the locus of the control and the programmed locus of the goal stimulus. A single signal thus represents the S-event (Rs) and E-event (goal-stimulus input). Because the feedback consists of the difference between these two, it is difficult for the naive operator to dissociate the effect of R from that of the program [Chernikoff, Birmingham & Taylor (70)].

The typical tracking task involves at least two, possibly three, primary sources of information. The display produces the first, the control R the

second. Both of these are intrinsic loops for the system in that there is no system without them. The display loop is external, whereas the control loop is proprioceptive and internal. Bahrack (32) and Noble & Bahrack (169) review their own excellent work and that of others on the difficult problem of the proprioceptive loop. Gordon (104) evaluates tracking performance under the impoverished conditions of no target, no follower, and standard conditions of pursuit and compensatory display. The third loops, those not directly originating in R and display, are more expendable, extra, or supplementary. Lincoln (142) compares the learning achieved under different varieties of extra loops.

When feedback involves a function of R, there is no such thing as the KR for a particular response. Probably all external feedback is arbitrary inasmuch as E calls the tune (154). This has been amply shown for simple, discrete response-learning systems (50); and for tracking tasks with cathode ray tube, it is quite obvious that E decides the amount of magnification and other properties of the feedback (42, 110). Or, in the classical line-drawing task, imagine that S's pencil moves two inches or two glubs [the unit actually used by Denny *et al.* (75)]. If we let KR equal  $f(X)$ , then KR is equal to whatever E wishes it to be:  $aX$ ,  $aX + b$ ,  $X^a$ , or  $a\sqrt{X}$ , etc., even  $X$  itself. Notice that KR may, with great propriety, be a "hit," "one glub," "nine glubs," etc. Those Es who round off a KR have, in effect, given up the notion of "true KR." Some transformations are interesting, practical, or analytic; others are of limited or no value (55).

Feedback, KR, reward, and reinforcement are usually cross-indexed in textbooks; too often, only studies of reward with animals will be found. Many skills people use reward and KR interchangeably, others use reinforcement and KR as equivalents. To name only a few: Saltzman, Kanfer & Greenspoon (191) speak of "delay of reward" for wrong and right in line drawing, Noble & Alcock (161) use "delayed reward" to denote the delay of the green light on their switch-pushing task, and Reynolds & Adams (182) use the word "reinforcement" to represent the click event when their rotor stylus is on target. The writers (58) have discussed, though much too briefly, the similarities and differences between occurrence and nonoccurrence of KR and food and have found sufficient reason to believe that, in almost every case, E's use of reward with his animals differs substantially from his use of KR with human Ss. This does not invalidate the work on either side of the fence, but it does mean that there is no special virtue in generalizing theoretical similarities *ad infinitum* in the absence of data obtained under comparable procedures. Identical procedures will not be easily come by, however, because "23 units low" was not evolved as a substitute for the function of food. When feedback is an error event, the prior R is weakened. This type of feedback certainly has a function more in common with no food than with food. Another difference lies in the

typical R—animal Ss seldom being taught skills such as how to track and move.

Rewards have been varied in a thousand ways, and temporal-frequency schedules can be generated endlessly, as Skinner's work shows so emphatically. Theories of reward and reinforcement run not too far behind. We seem to be in for the same empirical and theoretical treats in the field of KR, for many variations in reward (155) can be copied with KR. If anything, KR variations will prove to be much more numerous; they may even, in turn, lead to some attempts at parallel manipulations with animal reward.

The outstanding thinking on KR in the 1940s was done by Brown (69) who discussed three now-famous roles of KR: reward, information, and motivation. That is, like primary reward, KR might serve to reinforce (strengthen) habits, evoke already established habits (cue properties), and provide the motivation (incentive) for learning or performing. These ideas were generalizations from the issues of reward research, and even today there is no methodology to differentiate between the alleged effects. It must be said, however, that KR research is not yet overly concerned with theory, since it is more or less acknowledged that suitable probes are wanting. Identifying relevant variables and finding functional relationships are much more militantly pursued. Brown also made 12 statements on variables and their probable effects. The first four concern the delay of feedback. For example, he identifies certain conditions where delay of KR might be superior to its immediate administration. This may be a bit of foolishness to those skills investigators who confuse KR with food; but it shows that Brown can tell a rat from a human being, and one experimental variable from another. Other statements deal with the specificity of KR, the effect of interpolating activity between R and KR, and independent scheduling of R and KR frequencies. Reward parallels of these last two have been hot topics in animal research for some time, but are comparatively cold for human KR (61, 58).

Ammons (15) provides the only summary of KR research during the 1950s. After reviewing 56 articles, he formulated 11 generalizations and eight summary statements as a beginning theory. These are a melange of intuition and conservatism and well represent the chaos and achievements of the field. He stays clear of definition, except to say that studies of level of aspiration and of incentives with human Ss are not included. Some of his generalizations are good: "In the case of discontinuous tasks where knowledge of performance is given, small intervals between trials are generally better for learning than are longer ones." Many others are vague, even if they make a certain amount of sense: "The more specific the knowledge of performance the more rapid the improvement and the higher the level of performance," and "For all practical purposes, there is always some knowl-



edge of his performance available to the human performer."

*Ineffective delays of KR.*—Ammons' Generalization 6 states: "The longer the delay in giving knowledge of performance, the less effect the given information has." The clunk of the pellet dispenser is loud and clear: "It is quite possible that the learner might not be able to use the information given more than 15 or 20 seconds after the response. . . ." It would have been better had these been supported by reference to the literature on delay of reward and by equating food and KR, as Wolffe (225) made no bones about doing in an earlier review; Ammons actually cited studies with human Ss, most of which were null. Among these was the classic by Lorge & Thorndike (143). Actually there are many other studies with no difference between immediate and delayed KR: Saltzman, Kanfer & Greenspoon (191), Noble & Alcock (161), and Denny *et al.* (75). McGuigan (149), too, had a null result, but because of design naivetes and strange statements, his data might well be discounted. Greenspoon & Foreman (107) published a significant difference that Bilodeau & Ryan (61) were unable to reproduce. All in all, it is clear that to delay or to give immediate KR can be quite immaterial for learning to make relatively simple Rs (when the periods between Rs are relatively free of specially interpolated Rs).

The writers (59) reported the results of five studies (800 Ss) with unusually long intervals—hour, day, or week—between successive Rs and between R and KR. The major finding was that the more massed the Rs the better, a result in accord with an earlier study in England (74) and a follow-up study by Denny *et al.* (75). Generally, the temporal position of KR did not matter, except in one of the five experiments where the delayed group was best. Ryan (190), too, got no effect of delay in a study in which E progressively shifted the required amplitude of R, delaying KR a week or not at all for each of eight weeks.

The many failures to obtain significant differences cannot be explained by complaining that traces of R last but a few moments, or by insisting that Ss instruct themselves. The writers do not question the null data; they do doubt the value of the classical role of the trace of R. Traces of the last KR and of the effects of past KRs on past Rs (alternative Rs) are also involved. Delayed groups have some advantage from more recent KR; for example, "Lift the cross-hairs 3.75° higher than you normally would" may be delayed and used as an effective cue for immediate action. Delay of KR is not necessarily bad.

*Terminal KR and instructions.*—Miller (153) and Annett & Kay (25) use the term "action" feedback to contrast with "learning" feedback. If action feedback has the cue properties of a conditioned stimulus, it has something in common with instructions and commands to perform. Feedbacks such as "Pay half as much attention as usual to display-control A, twice as much attention to display-control B," have been used sparingly (54). They alter the part-performance profile for a time, as does announce-

ing "Your poorest performance is 'throttle'." Similarly, Goldstein & Rittenhouse (103) tell S the sign of his constant ranging errors and get improved performance on a gunnery trainer. Telling S which switch is paired with which light speeds learning [Noble, Alcock & Frye (163)].

Work on continuous, action feedback has been going on for years, but it is difficult to find it compared to learning feedback in the same experiment. Using a pressure stick, Annett (24) compared visual feedback while R was in progress to announcing the result after R was completed. The terminal KR he calls "delayed KR," the action feedback he calls "immediate KR," though it is not as simple as the names imply. Removing the visual feedback during the test period produced a quick and dramatic fall in the accuracy of the visual group, but not the terminal group. During its training, however, the terminal group had improved gradually; the visual group hit the target every time. Obviously, the delayed group learned something that the other group did not. Annett is sorely troubled by the lack of learning in the visual group. Among several alternatives he suggests is that S has learned to move the light and when this visual cue is removed he cannot reproduce the pressure he has not attended, i.e., learned. The results of Pearson & Hauty (174) really pose a difficult explanatory problem since their Ss learned to right themselves in a lateral tilt chair without any identifiable external sources of feedback. The improvement they attribute to proprioceptive learning and not to sensory adaptation. Holding (113) maintains that in full guided practice there is no KR, only knowledge of the correct R and that such kinesthetic cues as there may be are irrelevant. More cases of no learning with KR and learning without KR can be expected in the 1960s along with increases in the loudness of claims and counterclaims.

*Effective delays of KR.*—Not all delays of KR produce null results, as delays of speech or handwriting (216) show so dramatically. It depends upon what happens between R and KR. In all of the preceding studies of delay, S learned simple, discrete positioning movements such as line drawing and lever moving. Nothing special was interpolated between R and KR or between KR and the next R. When something is interpolated, performance is of a different order of magnitude, and Es such as Noble & Noble make learning as difficult as they wish (167). There is a whole spate of evidence, most of it in the context of human-factors engineering. Representative studies of display and control lags, without large overlays of learning theory, are those of Garvey, Sweeney & Birmingham (99), and Conklin (72, 73). Holland & Henson (114) show that performance improves immediately upon a change from delayed to immediate feedback. Bilodeau (62), following Lorge & Thorndike (143), has used a trials-delay technique to effect a compromise between the tasks of continuous tracking and discrete positioning. In trials delay, discrete events run off about as follows: R1, R2, R3, KR1, R4, KR2, etc. This sequence is illustrative of

but one simple type of delay with serious consequences for learning and performance.

In the much too typical tracking task, S continues to track during a lag, for his flow of information is still continuous—merely displaced in time. Habituated to sampling on-going action feedback, S adjusts the control as if that feedback represented his on-going error. In terms of system output, of course, this is all wrong, and more wrong, the longer the lag. As in steering a car, airplane, or submarine, one must anticipate. On-going feedback must be interpreted as a function of Rs preceding the immediate past and as cues related to future events.

*More of verbal and mechanical instructions.*—Instructions and tuitions have the appearance of belonging to the feedback family and are administered before, during, or after R. Below, we deal with the more derivative feedbacks, from initial verbal instructions to S, through feedbacks while R is in progress, to feedbacks summarizing preceding responses. The topic is also divisible, but unnaturally, into studies that make use of verbal instruction and those that employ mechanical surrogates for the spoken word.

Everyone knows how important "motivational" instructions and environments are, yet most Es who undertake a demonstration come up with null results or the thinnest of data [Bayton & Conley (44); Bell (45); Fleishman (95); Noble (158, 166); Williams (224); Zimny (227)]. Others get positive results (205), and the most important of these is Wegner & Zeaman (222). Walton & Begg (220) found that incentives improved the performance of imbeciles on a dull routine task. The whole area of motivation is a dreadful mess with no promising way of straightening it out. Too often, one group is advised to "try hard" and a second group to "try very hard." It is also fashionable to award a nickel, a gum drop, or to threaten a weak shock; it is not the thing to give your Ss KR or instructions such as "stop walking and start running." Surwillo (208) lists nine techniques alleged to vary motivations in the laboratory. Still, he misses some. French (97) has good ideas on individual differences and finds that performance is more closely related to motivation test scores than to experimental conditions.

One can give S some extra indication of decent or poor performance beyond the standard without using "good," smiles, or cookies; the color of the target, a tone, or a special lamp can convey an additional bit of information about on-off target events. The extra KRs that work best are those that attract attention (warning lights) and those that carry relatively specific information about which R has gone wrong. Vague and redundant KRs are readily produced and just as quickly attached to training devices.

Payne & Hauty (172) showed S his relative standing, or a light to signal that an error was in progress. The Ss, under the influence of analeptic or depressant drugs, spent more than four hours at four-dimensional tracking on a splendid apparatus, the Multidimensional Pursuit Test. The Ss paid

their toll, but the feedbacks helped performance throughout. The feedbacks, however, were no better at sustaining performance late in practice than early. Hauty & Payne (111) show very nicely that extra signals will serve to raise the entire performance curve during a hard day's work under trying circumstances. Showing better performance with extra cues is not the same as demonstrating greater learning. The training people naturally hope that the extra stimulus will promote learning of the standard task (transfer of training). The unwanted alternatives are that the extra cue (a) elicits on-target Rs with no particular transfer to the standard cues and (b) motivates S to emit Rs at a greater rate, and thus, of course, to perform better only with the extra cue (i.e., increased motivation), but not without it. Obviously, the learning-performance distinction is the heart of the matter.

Studies shortly after World War II used filters to redden the normally white target of a gunnery trainer. The filters operated when S was on target and they greatly improved performance. Later, switching to no-filter feedback showed that removing the cue produced an immediate and large letdown. There was no large amount of positive transfer, although there was usually some. An experiment by Annett (24) typifies the techniques now in routine use: the extra cue is withdrawn after training; during training its frequency is varied, patterned, or randomized, in an attempt to shift the properties of the extra cue to the standard ones (103). Stockbridge & Chambers (204) also did work of this sort, but McCormack (148) did not bother to use a test period. Smode's (201) discussion and ideas on extra KR are especially good. In his study, groups with extra KR were deliberately loaded with several supplements. As usual, there were large differences during training and rapid shifts in level of performance with transfer testing, but there was more than the usual evidence for some transfer. Smode says S is not learning more, but is being motivated to do more. Very wisely, he also included groups switched from one model to another. The combined effect of changing model and eliminating the extra cue is always greater than the loss attributable to either one. Apparently, the extra cue teaches the idiosyncrasies of the training unit, and this finding adds another limitation to training devices with or without extra cues.

The results of Archer, Kent & Mote (29) on a gunnery trainer and of Archer & Namikas (30) on the rotor are different in that the extra cue did not even produce a performance difference during training. The study on the rotor was a badly needed check of an earlier one by Reynolds & Adams (182), who found positive training differences using a delayed cue or tone to indicate that Rs for the last *X* seconds were correct. Archer & Namikas used college Ss; Reynolds & Adams, basic airmen trainees. The reviewers think the latter study a fluke, although Archer & Namikas prefer the sampling population hypothesis.

Studies with extra KR can be expected more often, with disagreement about their reinforcing properties. There is, however, no dispute about

certain extra KRs producing improved responses. Someone can be expected to use this technique to teach his Ss more about the criterion task. In the meantime, we can expect a number of failures.

*Numerical transformation of KR.*—When S's error or response ( $X$ ) can be expressed numerically, a transformation of the number,  $f(X)$ , can be the feedback. By deliberate, systematic, and arbitrary manipulation of these numbers, E can vary and regulate feedback to study the relations between R and KR. Transformations are standard practice with Es who use trainers and simulators because the electronic linkage between R and display must necessarily be arbitrary. Nearly all of the tracking literature can be organized around  $f(X)$ , all the transformations being either spatial or temporal in character (21, 42, 73, 209). In the section on Apparatus, Table 1 shows very clearly that most of the newer devices, electronic or not, are built so that E must decide the nature of  $f$  in  $f(X)$ . Noble & Bahrick (169), for example, use  $4(X)$  with their pressure stick; even in the line-drawing experiment, E need not equate KR with the marks on the ruler. The use of transformations of numerical feedback for discrete Rs was formally opened by Bilodeau in 1953 and was followed three years later by a survey of the transformation literature (55).

Commonplace transformations deal with such variables as rounding, target size, error amplification, specificity, or other ways in which R is coded. A transformation equation shows how the whole KR scale is related to the entire dimension of R. For example, when KR is set equal to  $aX + b$ , KR varies with  $X$  in a positive and linear manner, the value of KR depending upon the values of slope and intercept E selects. The subject's behavior can be regulated by variations within the equation as Schumsky (192) has done, or by variation of the type of equation as Noble & Broussard have reported (164). Noble & Broussard taught S to make a certain amplitude of R, using different curvilinear relationships between KR and R.

The independent variable, target size, is well handled by the transformation technique. The size of a target is fixed by transformations about some critical value of  $X$ , a number of quantitatively different Rs being converted to a single value of KR representing the goal or target. In such systems S can be told he is on target and yet scored as in error. Responses seem remarkably insensitive to rather wide variations of target (55, 106). When the target is wide, S usually repeats his Rs and avoids the edge [for behavior on moving targets see McConnell & Shelly (146)].

The major trouble with most psychomotor tasks such as line drawing is that too little learning is produced. Subjects do much too well on Trials 1 and 2, having a decent R for "quarter-inch error" and a pretty good three-inch line-drawing R before encountering the experimenter. The task is not worthy of human learning abilities. Transformations show promise for slowing or speeding the learning process as required for line drawing and

many other tasks. Transforming the error fed to *S*, by adding or subtracting a constant ( $k$ ), does regulate the speed of learning. In such an experiment (53), the sign and magnitudes of  $k$  have pronounced effects on certain variances and central tendencies. By comparison with  $-k$ ,  $+k$  had the effect of minimizing (*a*) mean absolute error of *R* and (*b*) individual differences, while maximizing (*c*) within-individual variance (i.e., hunting). The transformation  $-k$  had quite the opposite multiple effects. The implications are two: (*a*) the dominant effect of feedback is upon variances; (*b*) there is no way to minimize (or maximize) all criteria of response.

Our major conclusion on feedback is obvious: to control behavior, regulate functions of error.

#### MOTOR MEMORY

Memory is a word generally avoided by learning theorists. The principle of a relatively permanent learning factor, other things equal, is generally accepted; so a yes or no about the mere occurrence of forgetting rightly receives a chilly reception. Performance changes occurring with up to a day or so of rest are classified under recovery; their discussion is much more likely to include decay of inhibition, stimulus trace, and warm-up decrement than retention. If the rest period is occupied by responding on another task, the study will be classified under transfer and retroaction—improperly, perhaps, since most of us think that these provide our understanding of retention-forgetting phenomena. The best demonstration of this is the 1951 study by Lewis, McAllister & Adams (137) in which losses over five days without practice were entirely attributable to filling the interval.

To the extent that memory is not a long-term affair, any learning experiment could be construed as a memory study, but it hardly seems worth while. Where a rest intervenes between blocks of practice, 99 per cent of the literature is concerned with rests of no more than an hour or so and does at least as well without memory as with. This review treats only studies using a retention interval of a week or more. And these, unfortunately, have very little to offer, except for a few using rest as a logical tool: retention as an index of learning as opposed to performance (185); long-term dissipation of temporary work decrement (121).

*Survey.*—Among the few testing worth-while hypotheses were Lewis & Lowe (135) who used 14 training sessions and a three-month rest on the Complex Coordinator. The *Ss* showed so much resistance to forgetting that forgetting could not be demonstrated until *Ss* were divided into high-low scorers. Even so, the phenomenon was weak. The high group forgot more than the low group, who, in turn, forgot no more than with a day's rest. Neumann & Ammons (157), using a procedural task, inquired into the serial-position curve after periods up to one year—a decent query.



Recent research emphatically shows that motor-skills performance is not sensitive to the mere lapse of time. Battig *et al.* (43) found that there was almost no forgetting over a 223-day layover for a cathode ray tube tracking task. The four Ss had trained for over 100 days, the last 20 at asymptotic levels of proficiency. The authors picked up valuable information during training by asking and answering their own questions. Significant in their protocols were: "None of the Ss could verbalize the target-course in any way nor believed that he had learned the course," and the very much related proposition that "The learning consists primarily in the acquisition of skill in making adjustive movements of the control so quickly and precisely that. . . ." Failure to obtain forgetting may lie in failure to measure the skills.

Jahnke & Duncan (121) show the rotor a most unsuitable device for studying forgetting. This they did most adequately by using 440 Ss in tests from 10 minutes to four weeks after training. They found more evidence of gain than loss in performance with either massed or distributed practice. A few years earlier, Bell (46) got only a little warm-up-like decrement with a year's layover. Similar findings were obtained in a fine study by Ellis, Pryer & Barnett (88) with one- and 28-day retention intervals; they thought that mental defectives, if not normals, might forget rotor habits. The defective (mean IQ of 61) and normal Ss were compared on all the standard phenomena before and after rest. The defectives showed no less retention than the normals on relearning Trial 1, and by Trial 2, both groups had bettered their training performance. In view of all the evidence against the rotor as a device for studying forgetting, it is difficult to comprehend why Jahnke (120) chose this apparatus on which to waste a study of durations of practice and rest.

The one formal issue raised in the last 60 years, and generally left unanswered, is: Are motor skills really more resistant to forgetting than other types of skills? Mengelkoch, Adams & Gainer (151) say yes to the U.S. Navy. They say maybe to psychologists because two different classes of R cannot be placed upon a common scale. They used a flight trainer, two levels of training, and a four-month retention interval. Procedural Rs (125 of them) were more susceptible to forgetting than were flight-control Rs (few flight controls, but an unknown number of Rs). Their discussion of forgetting is much more worth while than most.

Surely, the prize for data collection efforts must go to Ammons *et al.* (18) for training 1000 Ss and retraining 650 of them up to two years later. They used continuous compensatory tracking as well as a procedural task and Ss trained to two different levels of mastery. The authors neither tell us what was forgotten nor compare the findings from the two tasks. Data for plotting a forgetting curve on the tracking task are not presented, perhaps because seven of the 10 groups failed to show any forgetting when reten-

tion was compared with training performance. Ammons, a well-deserved authority on the effects of short rests, was a natural for conducting the biggest forgetting study of the century, and the brevity of his introduction and interpretation underscores the frustration which research on forgetting usually brings.

*Comment.*—There are two reasons why so few studies of long-term retention have appeared, though the trend is rising. The first, easy enough to understand, is that it takes a lot of work to get S to report back. The second reason is that no one has advanced such worth-while hypotheses as to stimulate himself or anyone else. The typical questions sound so trivial: Will S forget with a layover? Will he forget more the longer the layover? When the right questions are finally asked, there will be a swing to retention-oriented research. The greatest single technical difficulty is making a showing of forgetting, and this necessarily postpones the issues of what is forgotten and orders of susceptibility.

In summary, then, we either dismiss forgetting as trivial on motor-skills tasks or introduce some questions and apparatus changes to answer what, why, and how. Certainly, retro- and proactive inhibition have not yet themselves presented us any final answer, in fact, if not in theory, as summarized by Lewis (134). The conclusion reached in the next section is that negative transfer (*a*) is difficult to produce, (*b*) when produced obtains in small amounts, and (*c*) rapidly converts to positive transfer. Every textbook writer knows the reason for forgetting is interference, everyone on the growing edge of the search knows that the interference data are nowhere as strong as the faith.

#### TRANSFER OF TRAINING

Because of our organization, many studies on transfer were covered in other sections. Several studies not reviewed deal with predifferentiation training and R-selection devices, and are summarized by Arnoult (31). There remains an unresolvable miscellany that we cannot handle, with one prominent exception—negative transfer.

*Negative transfer.*—A thorough 15-year search of variables producing interference and facilitation has been pursued by Lewis. He, Shephard, Adams, McAllister, and others at Iowa, were responsible for more than 50 studies analyzing negative transfer, or retroactive interference. The most useful design has been the famous OL—IL—RL or ABA paradigm, where the experimental task is interpolated between original training and relearning. The experimental task is usually an opposite control-display relationship; the apparatus, in earlier studies, a modified Complex Coordination Test, and lately, the Star Discrimeter (140), though other apparatus have been employed. In another design, verbal pretraining is used to establish transfer effects (145, 147).

This systematic work is badly in need of summarizing to prevent its loss or unwitting replication. Brief summaries of Lewis' interpretations were made by Barch (37) and Barch & Lewis (40) some time ago. An overview made in 1959 is not generally available (134). Major inquiries were made of such basic variables as level of original learning, level of interpolated learning, repeated alternations of OL and IL, apparatus, R measures, and methods of data analysis.

Lewis found only shards of the holy grail; it is not easy to produce interference (134). He shows impressively that its half life is a handful of trials. Reading chronologically, one gathers a growing dissatisfaction with the importance and theoretical status of interference. Lewis certainly found interference effects, but emphasizes the over-all positive trend with practice on either or both of alternating tasks (138, 139). Further evidence of the greater weight of positive over negative transfer is available in Duncan's demonstration of learning-to-learn (84). Lewis' dissatisfaction is over the comparatively small amounts of retroaction and the speed with which they disappear, the fact that negative will often convert to positive transfer, and the immunity of some measures of R to negative effects. As the crowning finding, Lewis reports that an individual is not consistently susceptible to interference [see also Spieth (202)].

*Comment.*—There are many who believe in the transcendence of negative transfer, and who must now pipe down or put up the variables which will produce the alleged phenomenon. Quite properly, they will find fault with Lewis' every turn . . . his task . . . his use of opposites . . . his R measures, etc. In view of the breadth and depth of the Lewis work, however, no one can believe that a pot boiler will suffice to counterbalance his findings.

When the display-control relationships are reversed, Lewis reports simultaneous interference and facilitation, i.e., both number of errors and number of matches increase. This is entirely consistent with other observations of the nature of skilled performance. In its very simplest expression it means that S is learning to dance around the target and to correct errors more and more quickly as they appear (43). Momentary and smallish errors may even be tolerated. After all, the stimulus for R may lie just as much in the error shown on the panel as in the stimulus light to be matched. Too, the dancing around may serve to provide a kinesthetic background from which more appropriate Rs may be selected. Lewis, McAllister & Bechtoldt shifted S repeatedly from task to task and concluded, from both correlation pattern and factor analysis, that the same skill was being learned in original and interpolated learning (138, 139). Such hypotheses explain why any training is beneficial for almost any other set of circumstances, except for one, errors. They are consistent with the finding that, immediately after reversal of the S-R relationship, errors should rise.

## DECREMENTAL EFFECTS OF PRACTICE

The recent history of motor-skills learning is remarkable for the intensive 10 to 15 years of research devoted to the decremental effects of practice and their dissipation with rest. In the late 1940s and early 1950s, both Ammons and Kimble were systematic contributors to the area—they had a point of view and set about vigorously to test it. Reynolds, Adams, and Duncan, with their own viewpoints, did a sizable number of studies in the early 1950s, and many, including Archer, Ellis, and the Bilodeaus devoted much of their effort to R decrement.

*Generalities, overview, and background.*—An aggravation to anyone who has ever varied distribution of practice on a standard piece of hardware is the knowledge that somewhere, someone is using his findings to urge an innocent consultee to distribute the practice of his trainees as widely as possible. In sympathetic tribute to the injured, we labor the following points as a warning against wild generalization of the well-established benefits of distributed over massed practice. The benefits are accompanied by restrictions with respect to the kind of R studied, the definition of a trial, and whether we are concerned with learning or performance. (a) If a simple, discrete R (say drawing a line) defines a trial, massed practice leads to superior performance (59, 74, 75). (b) The easily obtained performance differences in more complex Rs do not mean much, if any, learning difference. Differences between groups are sharply reduced once the difference in intertrial interval no longer holds (12, 19, 41, 76, 79, 125, 185, 203) and shrink progressively with continued training under a single level of distribution (8, 183). (c) The most practical restriction, emphasized by Duncan (81), is that the benefits of spaced practice may not apply if practice times are not constant. If like amounts of experimental time are allotted, moderately massed S can outperform extremely distributed Ss, even without a test for learning (77). An applied recommendation in favor of the widest possible distribution of practice is probably never justified.

As to so much other research, Hull provided the impetus, and a theoretical framework, to the boom in decrement research. Hull's presentation of  $I_R$  and  $gI_R$  was not detailed, and the credit for our current thinking about the growth of temporary decrement goes to Ammons (10, 11) and to Kimble (126). The two publications by Ammons represent a high-water mark in their detailed description and explanation of postrest phenomena. In view of the theoretical origin of the interest in decrement, the general superficiality of the treatment of the numerous and excellent studies that followed is impressive, at least to these reviewers and to Adams (6). Adams presents a good case for rigorously testing explicit hypotheses, but might have gone further. Predictions are formulated in terms of momentary effective excitatory potential, whereas empirical tests are made in terms of observed responses. Either the R measure bears a linear relation to the excitatory tend-

ency, explicitly assumed by Zeaman & Kaufman (226) and implicitly by scores of others, or exact tests are inexact to the extent that nonlinearity occurs. All this makes it impossible to know if we have verified Hullian or neo-Hullian thinking, particularly since there are all shades of meaning to the same term; we greatly need a separation of  $I_{R1}$ ,  $I_{R2}$ , ...,  $I_{Rn}$  and a thorough theoretical reworking. (Example: we have at least Hull's  $I_R$ , Kimble's  $I_R$ , Ammons'  $D_{wt}$ , as well as deliberate avoidance of any construct term by others, most notably Reynolds.) In addition, of course, there are Hull's and Kimble's  $sI_R$ s, an extinguishable  $sI_R$  (76), and various self-pacing adaptations and adjustments (19, 48, 183, 185).

*Recent history (pre-1955 findings).*—During a 10-year period of intense effort, decremental effects of practice were explored over various apparatus, with different variables, and in long- and short-term studies. The work on intertrial interval was particularly thorough. The effects of work loading (63, 86) and of trial duration (127) were not as popular, though there are a number of reports of the effects of rests interpolated at various stages of practice. Massing, spacing, and distribution here refer only to the rest between trials; interpolated rest, or rest, indicates longer rests separating blocks of trials into separate sessions where trials within a block may be massed or spaced. The following summary is incomplete with respect to both data and contributors; specific references must thus be taken largely as examples and the findings as applying to tracking or other near-continuous responding.

Performance has been repeatedly shown a negatively accelerated, increasing function of present spacing (4, 12, 124). The relation appears non-monotonic (12), though intertrial intervals of the order of 15 min. (or days) have not been pushed to the asymptote. Both predicted (124) and obtained asymptote (4) are negatively accelerated, increasing functions of intertrial interval. Performance gains follow the same function (124, 128) over much of the course of learning, though late in practice the gains from trial to trial have been reported as independent of spacing (183). There is dispute as to whether long or short intertrial intervals reach their respective asymptotes earlier (4, 124).

Other research concerns the learning-performance distinction. Ammons (12) found no particular relation between performance in two later practice sessions and a wide range of intertrial interval in a first session. Other demonstrations that the effects of prior spacing are transitory are provided by Archer (26), Bilodeau (49), Reynolds & Bilodeau (185), and Starkweather & Duncan (203), among others. Adams & Reynolds (8) shifted groups of over 100 Ss from massed to spaced practice after various durations of massed practice, and all groups converged on the spaced control. Further, Reynolds & Adams (183), with 2302 Ss given instantaneous shifts in distribution in both directions, found good agreement with rational equations making no allowance for permanent decremental effects from massed

practice. Thus, the data have never given much support to learning differences—in either positive or inhibitory habits. In fairness, we mention that a number of investigators, their experiments reviewed by Ammons & Willig (19), doubted a simple  $sI_R$  from the start and attempted as much to demonstrate the absence of  $sI_R$  as its presence. Further, of the two, Kimble and Ammons, who had made the most formal commitment, Kimble stated as early as 1949 (128) that  $sI_R$  would develop only under extreme and long-term massing, and Ammons [with Willig (19)] frankly retracted his earlier formulation. Ammons & Willig and Adams & Reynolds (8) give good accounts of why the very earliest conclusions are contradictory.

There are any number of nonshift studies presenting performance curves for successive practice sessions (11, 130, 184). Ammons (10) called attention to the characteristic form of the postrest curve (particularly dramatic for rotors, massed practice, and the first few rests) in a detailed analysis of the decremental and incremental factors in both practice and rest. Very grossly: reminiscence, marked gains in early trials, and losses in later trials characterize the postrest curve for massed practice; a drop in performance over the rest and a curve with a definite break, though of over-all positive slope, are typical of spaced practice. Changes in performance immediately after rest, however, received most emphasis because of their bearing on the dissipation of  $I_R$ . Ammons (10), probably more than anyone else, stressed both the negative and positive effects of interpolated rest, and he and others plotted one or more of three indices. The first, warm-up decrement, shown in a steep initial increase in postrest performance or by an absolute decrement over rest, reflects the negative effects. The second measure, straight gain over the rest (classical reminiscence, if the gain is corrected for the expected intertrial increment), shows the net effect of rest. The third, less common index sums reminiscence and warm-up to estimate the positive factor, amount of temporary decrement dissipated (11, 118, 130). In general, of course, such a corrected measure of recovery gives the same trends as reminiscence, but exaggerated, and is not treated separately below.

Ammons (11) and Irion (118) made early demonstrations of the warm-up trends for some of the major variables, both reporting increasing warm-up as functions of durations of pretest practice and interpolated rest, and Ammons (12) showing warm-up to be a decreasing function of intertrial interval. There is some question as to whether warm-up increases and then decreases with ordinal number of the rest; both increasing (36) and decreasing (1, 184) trends have been found. Peculiarly, for an interference-set interpretation, Kimble (126) reported that reversed printing does not yield the sharp initial rise characterizing warm-up.

For a single major rest, gains from the last pretest to the first postrest trial (and reminiscence) have been widely shown as related to the several temporal variables (but not work loading). Recovery, at least for rotors and reversed printing, is an increasing function of duration of pretest practice,



nearly maximal for about three minutes and probably at absolute maximum after eight to 10 minutes of practice (8, 11, 118, 126). As interpolated rest increases, recovery at first increases and then decreases (11, 118). These two functions have been heavily stressed as indicative of how  $I_R$  accrues with practice and dissipates with rest. Reminiscence also should vary with distribution of practice, and does (12, 128).

Gain has also been related to the ordinal number of the rest in experiments using more than two practice sessions. Barch (36), Adams (1), and Kimble & Shatel (130), for example, all with pursuit rotor learning, reported that gains after massed practice were a negative function of the ordinal number of the rest period. Bilodeau (49) accounted for a like trend with a work task in terms of little change in starting rate from session to session and large increases in final rate of R. The same work task (52) yielded a standard, negatively accelerated relation between gain and duration of a first rest. The function shrank progressively with repetition of the work-rest cycle, as the trends for individual groups imply: gains plotted against ordinal number of the rest decreased for long rests and at first increased and then decreased for short rests. Analyzing the effects of successive rests is, as Ammons (11) warned, extremely difficult.

Recovery effects have also been demonstrated with transfer of training designs in which S practices with one hand before rest and with the other after rest (108, 119, 129, 188). The findings have, unfortunately, given rise to clouds of nonsense about the locus of  $I_R$ , though, as Grice & Reynolds (108), for example, have shown, the data can be treated consistently with other recovery data.

*Current contributions, 1955 to 1960.*—Decrement no longer takes up most of motor learning, but still takes a good share. In addition to the studies reviewed below, many are treated in other sections as relevant to feedback, retention, abilities, and personality.

Teghtsoonian & Shephard (212), with a modified two-hand co-ordinator, tested, among other things, conditioning the resting R to a tone. Denny, Frisbey & Weaver (76) used the rotor and a good experimental design in which Ss had various combinations of massed and spaced practice over three sessions. Their view of the gradual performance adjustment after shifts to wider spacing is that  $sI_R$  is as extinguishable as any other habit. As evidence that the habit is there, they point to two groups having massed practice in the third session after spaced practice in the second: of these the Ss massed in the first session showed steeper decrement than the Ss spaced in the first period—as expected, if these two groups represent reacquisition of  $sI_R$  vs. its acquisition. The number of Ss is small, however, and Ammons & Willig (19) report the opposite effect, comparing a group shifted to massed practice with another group massed throughout. The best support for the kind of effects Denny *et al.* predict is in the pacing—or conservation—effects sometimes obtained from instructions about the amount or length of work S

faces. Conservation may not be easily found in learning tasks with low work loading: Noble & Cieutat (165), with two-hand co-ordination, found that time-set failed to cue either initial or over-all performance.

Following Ammons' (13) earlier attempts to analyze rotary pursuit Rs and growing stress on underlying skills, several studies combine standard treatments with analysis of subskills. Archer & Bourne (28) have a very good analysis of travel and printing time as functions of sex and spacing for reversed printing. The most striking finding was the sizable effect that intertrial interval had on the time required to travel from printing one letter to the next, as compared with printing time. With motion pictures, Ammons, Ammons & Morgan (17) took a good look at the general patterns of behavior in skilled as opposed to beginning Ss, whereas Bourne & Archer (66) and Archer (27) took durations of hits (time continuously on target) as functions of spacing of practice. These studies show that durations of hits, the time Ss spend making rotary movements, or both, increase with practice and with intertrial spacing. Ammons *et al.* (17) object to a literal resting R, since Ss do not stop the stylus. Archer's point is that essential skills may suffer from lack of practice under massed trials and that measures of R patterns characterizing skilled performance may show how and where, a view Digman (78, 79) adopts in a pair of shift studies of rotary performance measured with time on target.

There are a few follow-ups of bilateral reminiscence, or relevant manipulations. Albright, Borreson & Marx (9) tested their version of Hull's effector localization hypothesis of  $I_R$  according to the activity interpolated between learning trials: rest and same- or opposite-hand activity; they urge a central inhibitory factor. Walker, DeSoto & Shelly (219) attempted unsuccessfully to produce bilateral transfer of warm-up on rotary pursuit. Adams (5) has an ingenious experiment demonstrating that merely watching the rotor can be detrimental. He assured active watching by requiring S to signal when his working partner was on target. His discussion considers the component Rs entering any psychomotor task. Duncan (83) was successful in producing decremental effects from active rotary movements without visual stimulation, and unsuccessful with passive watching. The findings are interpreted in terms of massed kinesthetic stimulation.

Barch (38), checking on the interaction of trial duration and intertrial rests, verified earlier findings that their performance effects are additive. Pearson (173) asks if subjective fatigue can serve to predict output; Wendt (223) says it is not a simple relation. Payne & Hauty (171) have the most pertinent data, comparing both excitant and depressant drugs and two levels of motivating instructions before extended tracking. Attitude was not affected by the depressant drugs and differed (at borderline significance) for instructions, whereas drugs affected motor performance and instructions did not. Attitudinal fall-off, or subjective attitude, thus, cannot account for their decrement in motor responses. Eason & White (85), on the basis of gen-

erally nonsignificant effects and as few as five Ss per group, propose a new two-factor theory in which muscular tension and fatigue contribute to determine performance. These authors cannot be said to have provided the thorough thrashing out that the question of  $I_R$  vs. fatigue deserves and needs.

*Current comment.*—Our arbitrary break at 1955 is an awkward one, occurring somewhere in a period of transition for both emphasis on decrement and its interpretation. For proof of the transition, we cite a paper by Ammons & Willig (19) as marking an important reshuffling of the positions of the three major, earliest neo-Hullians in this area: Ammons, Kimble, and Reynolds. More generally,  $sI_R$  fell into wide disrepute, and while  $I_R$  is still employed, there has been increasing use of such terms as stimulus inhibition, neural satiation, and central inhibition in both experimental and theoretical reports [Thompson (213); Walker (218)]. For human motor learning, however, one of the  $I_R$ s should stand until some other term demonstrates that it is more than a euphemism.

Alas, it looks as if residual effects are still with us, though the responsibility for them has shifted from inhibition to habit. The current claim is not that massed Ss learn a negative habit, but that they do not learn the skill—a view at least as old and legitimate as  $sI_R$ . But the powerful, now-elderly data of Reynolds & Adams (8, 183) show that it will not be easy to find dramatic, lasting postshift differences. That skill differences are not easily come by is evident also in findings for severe pacing, which can also limit practice on component skills [Adams (3); Anderson, Kresse & Grant (20)]. A new study ought, at least, to go beyond the Adams & Reynolds range, state and measure just what is the skill that suffers, and use something other than rotors. If the magnitude of earlier phenomena and the conditions needed to produce them are ignored, we are in for a wasteful proliferation of studies presenting the old data with a new interpretation.

#### DIFFERENTIAL STUDIES

A fair-sized body of literature is extending the range of skills phenomena to populations beyond the airman and college student.

*Mental defectives.*—The work with low IQ Ss, particularly, contains a very evident learning interest. Gordon, O'Connor & Tizard (105) assure us that imbeciles make good Ss; their own research on incentives bears out their claim. A number of experiments, for example, by Ellis & Sloan (89) and Ellis, Pryer & Barnett (88) on rotors and Reynolds & Stacey (186) and Ellis, Barnett & Pryer (87) on mirror tracing show a relation between mental age and performance. Distefano, Ellis & Sloan (80), with a battery of tests, obtained some evidence that sensorimotor deficit accompanies intellectual deficit. Barnett & Cantor (41) used the transfer design and a rotor to demonstrate standard distribution of practice phenomena, including the absence of learning differences.

*Personality.*—Research on manifest anxiety is usually reviewed elsewhere; so we ignore it. Miles (152) has some suggestive findings for habitual, general ways of approaching tasks and for measures of achievement-motivation as variables in original learning and relearning. Eysenck (90) has triggered a number of studies failing to relate decrement and extra-version-introversion [Bendig & Vaughan (47); Ray (178); Rechtschaffen (179)]. Bendig & Vaughan take a medal for replicating their study, before publication, to check on the possible interaction of a pair of personality variables; the replication used even more Ss and gave no hint of interaction. Rafi (177) reports normal Ss superior to mental patients in motor performance (normals were younger), with chronic schizophrenics inferior to other patients, while Venables (217) shows that functional psychotics do not differ from normals in susceptibility to  $I_R$ . Examples of the uses to which  $I_R$ s are being put are Hunt's article (117) proposing an  $I_R$  account for the poor performance of high-drive Ss on difficult tasks, and the study by Malmo & Wallerstein (144) relating rigidity to weak  $I_R$ .

*Organismic variables.*—There are a few studies of handedness (206) and more of age, sex, or both [Ammons, Alprin & Ammons (16); Archer & Bourne (28); Botwinick, Robbin & Brinley (65); Shephard (195)]. In general, when interactions with age or sex are explored, the major phenomena are found for all ages and both sexes. Kirchner (131) has a good investigation of age  $\times$  task interaction, comparing older Ss (60 to 84 years) with younger (18 to 24 years) on a light-key apparatus. He relates the ineffectiveness of the aged on difficult tasks requiring R delays to organizing abilities, rather than to motor speed.

#### OVERVIEW

Motor-skills learning has made enough progress in the last 10 years to require a period of consolidation. We venture our necks to extrapolate the following trends in research. Increased efforts: feedback, memory, intratask composition rules, reviews and theories, application of skills techniques in clinical psychology. Decreased efforts: response decrements and recoveries, proliferation of classes of apparatus, complex trainers, simulators, and rotors.

Our nominations for raspberries in the annual awards program are:

1. Skill is a within-subject, not a between-subject, phenomenon. There is, thus, a limit to the information that comparisons of Ss or groups will give. Most studies of skill have handled between-subject phenomena.
2. There is more to learning than the first few trials, and there is too little effort in the study of asymptotes and skilled performance.
3. There are few surveys of data or theoretical reviews.
4. A reverence for bumps and wiggles among Rs attracts a considerable body of pilgrims. A few examples will have to do: the residuals after shifting from one level of distribution to another; the difference produced by in-

forming S that he is in the sixtieth percentile, not the fortieth; the correspondence of the galvanic skin response and standard measures of response; the difference in learning between a 2-sec. and a 10-sec. delay of "right" and "wrong."

5. The transfer or shift design to separate learning from performance remains the only technique bearing on the matters of supreme consequence.

6. Lip service is paid to motivation as an explanatory factor, whereas, in fact, it is the most useless part of our comparative inheritance.

Recent years have brought some favorable developments, and surplus rotor parts are available to serve as incentives for further work:

1. Increased sophistication in stimulus and R variables for a more accurate specification, regulation, and variation.

2. The expression of many functional relationships and other low-order laws from which theory may properly proceed.

3. Achievement of an independence from rat and reward psychology which no longer provides most of the initial hypotheses, methods, and theories. This also frees the rat psychologist from the pain of seeing his ideas progressively mangled down the line and of being faced with irrelevant crucial tests of his predictions.

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## BRAIN FUNCTIONS<sup>1,2,3</sup>

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The output of studies on brain mechanisms and behavior continues at such a high rate that it is impossible within the limits of this review to survey all the areas pertaining to this subject. Consequently, no systematic survey will be made herein of motivation, emotion, or thinking, nor of the literature in clinical neurology and neuropharmacology.

Exploitation of the method of implanting electrodes in the brain of unanesthetized animals has given much impetus to research in two areas: one deals with the study of behavioral effects of electric stimulation of various brain structures, the other with a search of electroencephalographic (EEG)<sup>4</sup> correlates of behavior. Of special interest is the growing literature on the application of these techniques to conditioned response studies. The methods of ablation and transection have been used with greater sophistication to analyze the functions of cortical and subcortical areas. Recent electrophysiological findings on the specific sensory systems have added apparent complexity to their mechanisms of action, and many anatomical and behavioral studies, especially on sensory learning, have been directed to their elucidation. These general topics, then, are the ones being reviewed. The review articles by Rosvold (187), Olds (154), and Neff & Goldberg (149) antedate the present paper.

Students of brain function have suffered a great loss in the death of Karl S. Lashley on August 7, 1958. Lashley's contributions to the study of neural mechanisms and behavior are among the greatest in this century, and his experimental works and theoretical critiques are of enduring importance. His last published paper on physiological analysis of higher mental functions (117) clarified the central issues and proposed a theoretical approach. Hebb (90) has written a penetrating evaluation of Lashley's contributions to science and of him as a man.

<sup>1</sup> The survey of the literature pertaining to this review was concluded in March, 1960.

<sup>2</sup> The following abbreviations have been used in this chapter: EEG (electroencephalogram); CS (conditioned stimulus); CR (conditioned response); US (unconditioned stimulus); UR (unconditioned response); p.s.p. (postsynaptic potential).

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<sup>4</sup> For convenience, the term EEG is used here to include both electrical recordings from the scalp by conventional silver-disc electrodes and recordings from the cortex and subcortical regions by either acutely or permanently implanted electrodes.

## BOOKS AND SYMPOSIA

The first two volumes on neurophysiology in the *Handbook of Physiology* (68, 69) appeared in the last year. These volumes and the forthcoming third will undoubtedly be standard references for workers in the field for years to come. The chapters on brain potentials and sensory systems in the first volume, and those on the motor mechanisms and central regulatory systems in the second, are relevant to the topics of this review.

The first two transactions of the *Conference on the Central Nervous System and Behavior* sponsored by the Josiah Macy, Jr., Foundation (19, 20) contain much informative material in spite of an unwieldy format. The historical survey of Russian neurophysiology, including sketches of Sechenov, Wedensky, Pavlov, Bechterev, and others, is a timely reminder of America's neglect of the Russian literature. Bureš developed a technique of using spreading depression to reversibly block function in the rat cortex. As described by Rusinov, nonspecific EEG waves at the human vertex develop during conditioning at a time when a conditioned stimulus is expected, but fails to appear. It is as if this neural event substituted for the absent conditioned response. These items serve as samples of research on brain function in Eastern European countries. Discussions of the work of Doty, Galambos, John, Morrell, and Worden on electrical correlates of conditioning, and of Sperry on interhemispherical transfer of learning gather together much of the current activity and thinking in these rapidly expanding research areas.

A number of symposia dealing with brain mechanisms and behavior were held between 1955 and 1957 (8, 86, 184, 201, 206, 232). It is impractical to discuss in detail or even to list the contents of these volumes, other than to record their existence. Walshe's (228) trenchant discussion of the current vogue of symposia represents an extreme point of view, but is not without some truth. The Wisconsin Symposium (86) and *Behavior and Evolution* (184) contain systematic presentations of specific topics providing useful, comprehensive surveys; the latter includes chapters on evolutionary aspects of brain functions. *Reticular Formation of the Brain* (102), a sequel to the earlier book on *Brain Mechanisms and Consciousness* (43), consists of many unrelated technical papers. In this new volume one fails to find any coherent objectives in the papers, nor is there any systematic examination of the central issues such as those exemplified by the papers of Fessard and Lashley in the earlier volume.

Magoun has written a systematic evaluation of research on the reticular system (128). A large amount of information is succinctly summarized in this volume. Penfield's Sherrington Lecture is a general discussion of his observations on cortical stimulation in conscious human patients (160). The book on speech and brain mechanisms by Penfield & Roberts (163) includes a useful summary of aphasia cases in the literature. Their own material indicates that electrical stimulation of the motor areas in either

hemisphere produces vocalization, but that disruption of speech occurs only from stimulating the left Broca's, supplementary motor, or temporo-parietal areas. Two monographs on sensory aphasia should be noted (95, 111). In both instances the authors had the rare opportunity to study the brain sections of their cases. Hopf's work (95) is of special interest. He subdivided the temporal lobe into 11 areas and attempted to localize six types of sensory aphasia (17 cases) in these architecturally defined areas. Nielson's book on memory and amnesia (150) and Russell's on brain, memory, and learning (193) are clinically oriented views on these subjects. For those interested in historical development, Riese's short history of neurology (180) is a convenient source. A recent symposium (169) gathers some interesting material, not generally available on the history of knowledge of the brain. There are papers on medieval and renaissance, Chinese and Tibetan concepts of brain and psyche, as well as on the more familiar concepts of Descartes, Jackson, Gall, Galvani, etc.

Two slender volumes provide refreshing reading. Sholl (200) has reported his Golgi study of the cortex along with a statistical analysis of the data. The electrophysiology of an isolated piece of cortex is described by Burns (29). In spite of the specialized nature of their reports, the authors are not unwilling to speculate from their data about how the brain works.

#### ELECTRICAL RECORDING

*The genesis of EEG.*—Attempts to correlate the EEG and behavior in unanesthetized, free-moving animals demonstrate orderly EEG changes related to behavioral processes such as habituation, conditioning, extinction, and differentiation in the conditioned reflex paradigm. Once a correlation is established, the search for neural mechanisms of behavior may proceed more directly through analysis of brain potentials. Such hopes would be justified if it were known what the EEG waves stand for. For this reason, the hypotheses on the genesis of EEG together with some recent related experiments are reviewed here. Articles by Bishop (14), Grundfest (83, 84), and Purpura (172) have surveyed the literature on this subject more extensively.

There is general agreement that the EEG waves recorded from the surface of the brain are not simply envelopes of axonal spike potentials, but rather reflect, at least in part, graded, summable, dendritic potentials. These dendritic potentials probably contribute primarily to the 15 to 20 msec.-duration surface negative waves which can be recorded from a given cortical site following stimulation of a cortical point a few millimeters away, stimulation of the homologous region of the contralateral cortex, stimulation of afferent fibers, or antidromic stimulation of the pyramids with recording from the motor cortex. These negative dendritic waves may be preceded by positive waves which are considered not to be dendritic in origin. The question of how such dendritic potentials are generated has



not been agreed upon. Chang (34) and Tasaki (213) proposed that they are action potentials conducted antidromically from somata toward their dendrites. Bishop (14, 39) considered them to be graded conducted responses and postsynaptic potentials (p.s.p.). Eccles (57) and Grundfest & Purpura (83, 172, 174, 175) maintained that they are chiefly p.s.p.'s. These last two workers, particularly, are strong advocates of the p.s.p. origin of dendritic potentials. Furthermore, they have argued that the dendrites are electrically inexcitable and can be excited only by means of chemical transmitters which presumably can either depolarize to set up excitatory p.s.p.'s, or hyperpolarize to generate inhibitory p.s.p.'s. In this framework the dendritic potentials are considered to be algebraically summed excitatory and inhibitory p.s.p.'s. The p.s.p.'s are standing potentials which are not conducted, as are action potentials, along an axon, but, instead, spread electronically. The oscillatory character of the EEG is thought to be caused by variously patterned impulses in presynaptic fibers setting up different p.s.p.'s, which are averaged and attenuated in the volume conductor of the brain.

Although evidence from eel electroplaques, muscle endplates, spinal-motor neurons, and gland cells indicates that these postsynaptic membranes are generally activated only by chemical transmitters (83), electrical inexcitability of cortical dendrites has not been directly proven. Nor has intracellularly recorded hyperpolarization of cortical neurons (164) been clearly shown to depend on chemical transmitters. Grundfest, Purpura, and their co-workers have, however, reported numerous experiments which lend support to these points. Among them are studies of effects on the surface negative waves (dendritic potentials), following (a) conditioning and testing stimulation of different afferent sources, (b) repetitive stimulation of afferent fibers, and (c) local or systemic administration of pharmacological agents. Since some of these drugs have specific synaptic actions at peripheral junctional regions (spinal motor neuron, neuromuscular junction, stretch receptor of crayfish, etc.), they may have the same effects on excitatory or inhibitory synapses of the brain. Grundfest and Purpura have used this pharmacological tool extensively to dissect the synaptology of the cerebral cortex, cerebellar cortex, and hippocampus. The significance of their conclusions depends on whether the surface negative waves, indeed, are summated p.s.p.'s, whether the dendritic membranes are electrically inexcitable, and whether the drugs used are specific to either excitatory or inhibitory synapses of the central nervous system.

In their earlier studies, Purpura & Grundfest (174, 175) showed that local application of strychnine increases the surface negative wave of the cerebral cortex, but has little or no effect on the EEG of the cerebellar cortex. On the assumption that strychnine is an inactivator of inhibitory synapses, they concluded that its effect on the cerebral cortex is to leave the excitatory synapses unopposed. These findings would fit in with the

concept that the cerebellar cortex has few, if any, inhibitory synapses. Systemic injection of *d*-tubocurarine in a heparinized animal increases dendritic potentials of the cerebral cortex, but decreases them in an animal treated with succinylcholine. This drug is therefore thought to be an inactivator of both depolarizing and hyperpolarizing synapses. Heparin protects the former and succinylcholine protects the latter. In recent publications, Purpura & Grundfest (83, 178) showed that cerebral cortical application of  $\gamma$ -aminobutyric acid will reverse the surface negative wave to a surface positive one. The normal blood brain barrier prevents this action of  $\gamma$ -aminobutyric acid when it is given intravenously (173). The notion that  $\gamma$ -aminobutyric acid inactivates the excitatory synapses is invoked to explain how its application reveals the effect of inhibitory synapses in enhancing surface positive waves. Purpura *et al.* (178) made a systematic attempt to correlate the structure of amino acids with their synaptic blockade action. The  $\omega$ -amino acids containing five or less carbons are classified as inactivators of depolarizing synapses, whereas those with six or more carbons are considered inactivators of hyperpolarizing synapses. These results have been partly confirmed by Takahashi *et al.* (211, 212) and Yamamoto, Yuyama & Iwama (234). Purpura, Grundfest, and co-workers (172, 176, 177, 204) have used this sort of pharmacological dissection to study the synaptic components of evoked dendritic potentials of cerebral and cerebellar cortices and in a study of the genesis of paroxysmal discharges of epileptogenic cortical foci.

The conclusions which Grundfest and Purpura have drawn from their concentrated effort to elucidate the genesis of dendritic potentials and the EEG have not been unanimously accepted. Some of the recent conflicting studies will be briefly noted. Grafstein (80) studied the surface positive and negative waves and the after bursts of transcallosal responses in an isolated cat cortex. She concluded that, at least in this preparation, these potentials actually propagate in three different fiber systems, and are not postsynaptic potentials. They can be independently varied by recording from different depths in the cortex, by local application of procaine, and by using different stimulus intensities. The effects on dendritic potentials of different intensities of cortical stimulation have also been studied by Brooks & Enger (21). They report a slow, direct and a fast, transsynaptic conducting system, but hesitate to assign them to p.s.p.'s. With regard to the action of *d*-tubocurarine, Ochs (152) showed that intravenous injection of this drug in cats (3 to 6 mg./kg.) causes a drop in blood pressure, and that when the blood pressure decreases to 40 mm. Hg there is a reduction of the negative potentials. A similar effect on the potentials can be obtained by withdrawal of blood to reduce the blood pressure. Thus, *d*-tubocurarine may not be a central synaptic blockage agent. Similarly,  $\gamma$ -aminobutyric acid may be a component of Florey's factor I, which is widely distributed in the mammalian brain (70); its central synaptic action, however, is in dispute. Iwama &

Jasper (101) and Goldering *et al.* (79) concluded that  $\gamma$ -aminobutyric acid has a general synaptic-blocking effect not restricted to depolarizing synapses. Local application of  $\gamma$ -aminobutyric acid as well as other agents (procaine, veratrine, boiling water) will reverse the negative dendritic potential to positive waves. Eidelberg *et al.* (59) also questioned the inhibitory action of  $\gamma$ -aminobutyric acid during EEG arousal. They found that intravenous injection of thiosemicarbazide depletes  $\gamma$ -aminobutyric acid in the brain, but does not affect EEG arousal. Finally, Kennedy (108) suggested that rhythmic characteristics of the EEG may be caused by mechanical oscillation of the brain resulting from pulsation of the choroid plexuses and cerebrospinal fluid. He reported a clinical case and used a bowl of gel as a model of the brain to demonstrate his hypothesis.

*EEG and conditioned responses.*—EEG changes have been reported to accompany Pavlovian and instrumental conditioning. The results vary somewhat from study to study. This variability may be partly attributable to the animals, the different conditioned and unconditioned stimuli used, and the different methods of training. Nevertheless, some general impressions emerge. The earlier literature has been summarized by Rusinov & Rabinovich (192), Yoshii and co-workers (235, 238), Buser & Roger (32).

Yoshii *et al.* (237, 238) studied both conditioned salivary and conditioned defensive reflexes in dogs, using a buzzer, light flash, or touch as the CS, and food or shock to the leg as the US. The CS is first given repeatedly until there are no EEG responses to the CS alone (habituation). After pairing of CS and US, the EEG shows generalized evoked potentials, generalized desynchronization or augmentation of fast waves in the cortical EEG, and 3 to 6 per sec. slow waves in subcortical leads in the amygdala, hippocampus, intralaminar nuclei, and reticular formation. As the conditioning continues, these changes become more pronounced. The behavioral CR always occurs later than the EEG CR. During differentiation the negative CS first elicits the EEG CR, later it induces 2 to 5 per sec. irregular, slow waves, which also appear during extinction. Similar results were reported by John & Killam (103) who studied conditioned avoidance response in cats, using flickering light as the CS. After the habituation procedure, pairing of CS and US induces rhythmic waves of the same frequency as the light flashes in the EEG in various regions such as the visual cortex, lateral geniculate body, reticular formation, etc. This photic driving response to CS does not appear in the amygdala or hippocampus. Later in conditioning, the photic driving of the first group of structures diminishes, but it appears in the latter group. When the animal makes 100 per cent CR's, only the nucleus ventralis anterior shows the frequency-specific EEG waves. However, the authors felt that EEG driving in higher harmonics also appears in the visual area, lateral geniculate body, and amygdala. When the animal makes a wrong CR to a negative CS, the EEG tends to have waves which are the same as those observed during response

to the positive CS. Galambos and co-workers (19, 88) found that in cats and monkeys the amplitude of EEG evoked potentials to clicks, intermingled with shock, increases during conditioning and decreases during extinction. These changes occur not only in the EEG of the auditory system but also in the caudate nucleus, hippocampus, reticular formation, etc. Widespread evoked potentials to tone bursts appear also in a food-reward situation (20). In this study, the evoked potentials disappeared, however, in all neural structures except the trapezoid body. Verzilova (223) found that the rhythmic EEG waves to metronome beats shift from the auditory to motor cortex during the establishment of a defensive conditioned reflex.

In contrast to rhythmic potentials synchronized with the CS as described above, other studies have shown the CS to cause generalized desynchronization or augmented-background fast waves in the EEG. The CS-US combinations used have been tone bursts and food; tone and strong, unavoidable shock; distension of the intestine and passive movement of the leg (11, 75, 121, 222). Furthermore, during the delay interval of a delayed conditioning process, clicks (CS) did not elicit evoked potentials, but caused a series of EEG changes, i.e., desynchronization, augmentation, and desynchronization again (76). Asratian (6) obtained different EEG amplitude changes in a dog in response to the same CS, depending on whether the animal was conditioned in the morning with a shock to the left leg (US) or in the afternoon with a shock to the right leg. Variations of the EEG waves in relation to the CS have also been demonstrated in different cortical layers of the dog (179) and in puppies of different ages (199). Grastyán *et al.* (82) reported that the 4 to 7 per sec. hippocampal waves at the beginning of conditioning changed into desynchronization in later stages. Any extraneous stimulus, any alteration of the environment that caused the animal to search around, brought back the slow waves.

In EEG studies in human conditioning experiments (104, 113, 185, 239), the CS induced either an enhancement of alpha rhythm or irregular slow waves before the administration of the US. Tone, light, and touch have been used as the CS, and light, passive movement of a limb, and verbal reinforcement as the US.

In summary, after repeated pairing of CS-US in the conditioning process, the CS elicits EEG alterations prior to the start of the US. The EEG changes may or may not persist, i.e., they may disappear when the occurrence of the CR reaches a high level. They occur in other neural structures, as well as the sensory and motor regions of the CS and the US. The type of EEG waves elicited (evoked potentials, slow waves, desynchronization, etc.) depends on the CS used, the experimental procedure, the environment, and the condition of the subject. Whether these EEG findings express the neuronal mechanisms of formation of new connections remains an open question. They may be reflections of the animal's attitudes (i.e.,

alertness, attention, indifference, etc.) which accompany the learning process rather than reflections of that process itself (19).

Two other types of experiment are relevant here. Yoshii & Hockaday (236) repeated the earlier studies of Morrell & Jasper (138) on conditioned EEG potentials. When a formerly neutral sound is repeatedly paired with a flickering light, the sound itself induces photic driving of the EEG at about the same frequency as that of the light flashes. Yoshii & Hockaday found that both reticular formation and nucleus centrum medianum are critically involved in this type of conditioning process. Rusinov (191) and Sokolova (205) studied the EEG during reinforcement of a dominant cortical focus. In their experiments, neither an anodal current applied on the motor cortex alone, nor a light or a sound by itself caused motor responses of the rabbit. But when a light or a sound was given during anodal current application, it elicited responses such as leg flexion. The background EEG obtained from the cortex and hypothalamus consisted of predominantly rhythmic, slow waves. There may have been EEG driving or a slight depression of the EEG amplitude of the visual and motor areas at the on-off points of the sensory stimulus. Otherwise, the EEG remained the same throughout each trial.

*EEG activation.*—Many studies demonstrate the general correspondence between activity of the reticular formation, EEG arousal, and behavioral wakefulness (43, 102). Other experiments assign more subtle functions, i.e., selective attention, to this system. Malmö (129) summarizes reports correlating the degree of EEG arousal to the level of performance on various tests in human subjects. He proposes that performance efficiency is low when the EEG shows synchronization. The level rises monotonically with increasing EEG activation, but decreases again with maximal arousal. He concludes that the reticular formation has no behavioral steering function. Studies by Hernández-Peón and co-workers on EEG habituation (33, 93, 94, 118) indicate the influence of the reticular formation in corticofugal control of sensory inputs. For example, following stimulation of the reticular formation, photically evoked potentials at the lateral geniculate body diminish in cats. Long (123) confirmed the effect of reticular formation stimulation on somatic and visual evoked potentials. The amplitude of evoked potentials decreased following high-frequency (250 per sec.) reticular formation stimulation, but increased following low-frequency stimulation (1 per sec.). In human subjects, the photic evoked potentials at visual areas decreased when the subject was asked to do arithmetic problems, to recall visual images, and so on (92). Kogan (112), on the other hand, interprets the EEG activation or hypersynchrony in terms of Pavlovian cortical excitatory and inhibitory processes. Thus, during the conditioning process, EEG blocking in response to CS is thought to be caused by increased excitation of the sensory analyzer, and whether a CS causes an increase, a decrease, or no change of the evoked potentials

depends, presumably, on the state of excitation and inhibition of the analyzers.

Many recent studies demonstrate the complex organization of the reticular formation, which is not a unified center having a global function. Cordeau & Mancia (40, 41) and Batini *et al.* (9, 10) report antagonistic functional centers in the reticular formation. A transection at the mid-pontine, pretrigeminal level induces EEG activation. This desynchronization changes into hypersynchronization if another transection is made at the anterior pontine level. Thus, a center for activation exists between these two levels, and the activity of this center does not entirely depend on afferent inflow. In cats with chronic midpointine transection, additional section of the olfactory and optic nerves causes transient EEG synchrony, which returns to activation pattern after one or two days. Similarly, increased intraocular pressure to block optic impulses induces transient and reversible EEG sleep waves (5). Furthermore, after unilateral midpontine transection, EEG activation is confined to the same side as the lesion. An additional cut at the medullary level eliminates this EEG asymmetry, a result which indicates a synchronization center at the caudal medulla. Enomoto (61) also reported this EEG laterality effect after unilateral stimulation of the nonspecific thalamic system. Tissot & Monnier (218) proposed a dual projection system in the reticular formation and diffuse thalamic system, which has reciprocal functional antagonism. Normal EEG activation in five comatose patients was reported by Loeb *et al.* (122). The lesions of three patients involved the midpontine region. Complete destruction of the rostral thalamus in chronic cats eliminated EEG activation only temporarily, but caused the absence of sleep spindles, and abnormal recruiting responses and barbiturate bursts (38). Magni *et al.* (127) reported EEG activation after intravertebral injection of thiopental sodium in *encéphale isolé* cats after the basilar artery was clamped. A transient inhibition of the synchronization center in the lower brain stem was postulated. The role of epinephrine and norepinephrine as chemical transmitters in the reticular formation has been questioned by Mantegazzini *et al.* (130).

*Experimental epilepsy.*—Chronic foci of EEG epileptic discharges in animals produce generalized convulsions as well as chronic behavioral disorders in the interictal period. Schmalbach (195) found that implantation of aluminum hydroxide cream in the temporal cortex of the cat causes restless and increased sexual behavior. When the alumina cream is applied to the occipital cortex the animals show increased irritability, incessant pacing, and visual attentiveness suggesting hallucination. Gastaut *et al.* (74) reported chronic jerking of vibrissae, turning of head, mastication, and salivation in cats with alumina cream implanted in the amygdala and hippocampus. The EEG of these animals shows focal spike, spike and wave activity, and slow wave in the interictal period.



Morrell (139) described the method of cortical application of ethyl chloride to produce a chronic epileptogenic focus. This method induces an autonomous, mirror focus of EEG discharge in the homologous area of the contralateral hemisphere. Schmidt *et al.* (196) studied single neurons in an epileptogenic cortical area. These neurons have higher frequency bursts than the normal cells during the interseizure period. It seems that such sustained spontaneous discharges are a fundamental characteristic of the epileptic neuron. An increased sensitivity of the neurons in an isolated piece of cortex to acetylcholine is reported by Echlin (58). Eidelberg *et al.* (60) stimulated the monkey's cerebral cortex to produce EEG seizure discharges and found that different cortical areas have different thresholds of electrical stimulation. Intrinsic differences of seizure susceptibility of the neurons in various cortical areas were indicated; this supports the usual clinical impression (161). The EEG seizure discharges induced by stimulating the temporal lobe have been reported to spread to many cortical and subcortical regions (85, 135, 165).

#### ELECTRICAL STIMULATION

**Conditioned responses.**—Early attempts to use electrical stimulation of the brain through implanted electrodes as the CS or US in establishing conditioned responses have not been conclusive (124). In 1956 Doty *et al.* (53) reported successful conditioning of foreleg flexion in cats, using direct stimulation of the marginal, suprasylvian and ectosylvian gyri as the conditioned stimulus. Appropriate controls insured that the spread of electrical current to meninges and blood vessels did not induce the conditioned response. The critical feature of their success seems to be the long interval (3 to 5 min.) used between trials. It is reasonable to assume that the cortex needs a long time to recover function after such strong, abnormal excitation. By using electrical stimulation of neural tissue as the CS and US, the search for the mechanism of formation of new connections may be simplified. However, almost all recent reports are still at the stage of demonstrating the formation of conditioned responses. Analytical studies by Doty & Rutledge (51) on the generalization of the CR point to the potentialities of this technique. These authors found that a CR (leg flexion), conditioned initially to a tonal or photic CS, could be elicited, at least a few times, by direct stimulation of the auditory or visual area without additional training. A CR conditioned to stimulation of one cortical point also responded to stimulations of the contralateral homotopic cortical point and some ipsilateral cortical points. Section of the hippocampal commissure and the posterior two-thirds of the corpus callosum prior to the conditioning training did not eliminate the interhemispheric transfer. Partial undercutting or circumsecting the cortex at the site of CS caused a transient loss of the CR, which could be re-established by further train-

ing. These results imply a functional equipotentiality of the intercortical or cortico-thalamic connections for this type of learning (19).

In an abstract, Doty & Giurgea (50) reported the formation of a CR by using cortical stimulation as both the CS and US. They used the hindleg flexion following motor area stimulation as the CR, and stimulation of the marginal gyrus as the CS. Mogenson (136) succeeded in establishing conditioned avoidance response in rats in a shuttle box by electrical stimulation of cortical areas 4, 18, or 17. Others used electrical stimulation as the unconditioned stimulus. Segundo *et al.* (198) stimulated the nucleus centrum medianum or reticular formation to elicit specific motor responses. These responses could be conditioned to the start or the cessation of a tone. Brown & Cohen (22), confirming the earlier study by Roberts (183), demonstrated the importance of the experimental situation in establishing conditioned avoidance and conditioned approach responses. They stimulated a hypothalamic point in cats as the US to form a conditioned avoidance response to sound (CS) in a shuttle box. By stimulating the same site in these cats as a reward, it was also possible to form a conditioned approach response in a straight runway situation.

Wyrwicka *et al.* (233) reported another type of experiment in which electrical stimulation was used to elicit a previously established conditioned response. They trained goats to obtain a food reward by placing the left foot on a food tray. After this response was well established, unipolar stimulation of the lateral hypothalamic area induced this motor response, even though the animal had been satiated with food. This study, together with the similar results obtained earlier by Grastyán *et al.* (81), revives the problem of localizing a center of conditioning to a discrete brain region. Wyrwicka *et al.*, however, interpreted the result to indicate that a conditioned alimentary response is elicited by the excitation of an "alimentary center"; the electrical stimulation would presumably activate the normal function of this center, in a manner similar to afferent excitation by the CS. Other studies have used electrical stimulation to disrupt conditioned responses. Nakao & Maki (148) showed that stimulation of the caudate nucleus in cats inhibited the conditioned avoidance response to sound in a shuttle box. Zuckermann (240) used electrical stimulation to inhibit a conditioned corneal reflex to metronome beats. He reported the absence of CR both during generalized EEG seizure discharges and up to 20 min. afterwards, following stimulation of the motor, visual, or auditory areas. Localized seizures in the visual or auditory areas had a detrimental effect, but seizures in the motor area did not prevent the appearance of the CR. Generalized seizure discharges in the reticular formation also inhibited the CR. Subthreshold stimulation of reticular formation, on the other hand, facilitated the conditioning process. These studies illustrate one of the fundamental problems in using electrical stimulation in unanesthetized

animals, i.e., whether it activates or disrupts the normal function of the neural structure stimulated.

*Self-stimulation.*—Olds (154) summarized the literature on the reinforcing effect of electrical stimulation of some neural structures. If shock was delivered to the hypothalamus, the rat would press a lever to receive the shock continuously for 48 hours without satiation (153). Stimulation of other structures (septum and amygdala) produced a decreasing rate of lever pressing within four to eight hours. It is not clear how this apparently strong rewarding effect of self-stimulation operates. The study by Nielson *et al.* (151) suggested that the electrical shock of the positive reinforcing points may induce widespread seizure like neural action. These seizures elicit the compulsory lever-pressing behavior. They reported that a weak caudatal stimulation could be used as a CS in a conditioned avoidance response, indicating that the stimulus was reacted to by the cat, but to elicit self-stimulation, a much higher intensity of the shock was needed. Furthermore, the stimulus had to be either prolonged or available in rapid bursts. These authors found that, in contrast to rat, the cat would not self-stimulate the septal area or the habenular complex. A species difference is clearly indicated. Porter *et al.* (166) recorded EEG during self-stimulation of various limbic and related structures in monkeys. Localized EEG seizures (spike, spike and slow wave, fast wave) were apparent in some cases after self-stimulation in the anterior hypothalamus, septum, anterior thalamus, hippocampus, but not in the amygdala. To this reviewer, their data suggest some relationship between EEG seizures and lever-pressing behavior. This correlation is quite notable after hippocampal self-stimulation.

Stein & Ray (209) and Ward (227) demonstrated that rats could regulate the stimulation parameters during self-stimulation to achieve a constant and, presumably, most preferred level. Ward concluded that the total quantity of electricity (number of coulombs) per shock is the determining factor; the frequency, pulse current, and pulse duration are relatively less important.

*General behavior.*—Implanted electrodes have been used to stimulate various neural structures in unanesthetized preparations but almost all these reports are limited to the question of functional localization. The following summary serves to record the wide range of phenomena disclosed by this technique.

Delgado (44) reported that the responses from stimulation of motor areas in monkeys were very stable throughout a four-year period. Also, the cortex displayed only minor histological changes, such as formation of capsule of fibroblasts, increased collagen tissue, and glia cells. Sano (194) found various somatic and autonomic changes as well as anger, fear, and searching behavior following stimulation of the deep temporal structures in cats. Motor responses were elicited from stimulating the cingular gyrus and nucleus medialis dorsalis (99, 201, 202). Stimulation of the cerebellar

vermis inhibited the motor responses elicited by motor cortex stimulation in cats, but an augmentation of flexion resulted from paraverminal stimulation (137). Kuroki (116) studied the arrest reaction following stimulation of reticular formation and midline area of the thalamus. During the arrest reaction, both the postural and nociceptive reflexes were present. The EEG showed either an activation pattern or synchronized large waves.

Many papers reported the stimulation of amygdala, hippocampus, and the septal area. In order to demonstrate functional localization of behavioral items and interrelations of these structures, Ursin & Kaada (221) and Fernandez de Molina & Hunsperer (67) described the different effects resulting from stimulation of different regions of the amygdala and adjacent regions in cats. Votaw (224, 225) stimulated the septal region or hippocampus in monkeys and reported many somatic responses. Cutting the fornix eliminated the responses from the former but not the latter. Brücke *et al.* (23, 24) described a "pacemaker" in the middle septum of rabbit. Direct shock to this region induces low-amplitude fast waves in the hippocampus and diencephalon. Mason (131) reported that plasma 17-hydroxycorticosteroid increased during stimulation of amygdala, as it does following hypothalamic stimulation.

#### ABLATION STUDIES

*Subcortical structures.*—Many subcortical structures have been implicated in higher mental functions. The brain-stem reticular formation, especially, is regarded as essential for consciousness, attention, and even learning. The two Symposia (43, 102) and the electrophysiological studies reviewed earlier attest to the large amount of work done on this subject. Unfortunately, the behavioral significance of the reticular formation has yet to be fully documented. The earlier studies by Lindsley, French, and co-workers (43) showed that cat and monkey became stuporous when the reticular formation was completely transected. These animals slept continuously and showed sleep EEG, but both could be aroused momentarily by strong sensory stimuli. These results were confirmed by Adametz (1), who obtained a different effect, however, when the surgery was performed in stages and the cat was allowed to recover between the operations. The rostral midbrain reticular formation was destroyed in four to eight stages. Such a cat showed practically normal behavior. Studies from Chow's laboratory revealed that cats prepared by this serial method could learn or retain conditioned avoidance responses and visual discriminations like normal cats. They also showed normal waking and sleep EEG's, and normal conditioned cortical repetitive potentials. Similar negligible effects were obtained following partial destruction of reticular formation in dogs and cats. Kreindler *et al.* (114) placed lesions in the ponto-mesencephalic reticular region of the dog and observed the effects on conditioned defensive response to food. Doty *et al.* (52) studied the effect on condi-

tioned avoidance responses of destroying the medial mesencephalic reticular region in cat. In both studies there was only transient suppression of CR's, which could be re-established rapidly. The latter authors found in one animal, however, that a lesion in the posteromedial hypothalamus abolished the CR. In contrast to these generally negative results, Hernández-Peón *et al.* (91) reported that small lesions in the lateral part of midbrain reticular formation disturbed a conditioned salivary response in cats. Lesions in midline thalamus, hippocampus, mammillary body, pretectal region, and cortex have no such detrimental effect.

The view that the limbic system is an important component of the neural substrate for motivation, emotion, learning, and memory provides the rationale for much experimentation. MacLean's (126) recent hypothesis is an example of attempts to systematize the functions of this system. One group of structures (lateral olfactory tract, amygdala, middle forebrain bundle, paramedian reticular formation, central grey) is said to serve for the preservation of self, and another group (medial olfactory stria, septum, cingular gyrus, hippocampus) for the preservation of the species. Effects of selective destruction of the structures within the limbic system are varied and perplexing; it seems not yet possible to assign any unified functional role to these structures. The fact that destruction of the hippocampus and surrounding structures impairs recent memory in patients (162, 226) has not been clearly reproduced in animals. Impairment of the formation or retention of conditioned avoidance responses has been reported after lesions of the dorsal hippocampus and its surroundings in rat (109, 214) and cat (100). That these postoperative effects are probably not attributable to a simple loss of recent memory or emotionality is indicated by the cat's failure to perform an avoidance CR to buzzer, contrasted with its avoidance of a flicker light, as the CS. Also, the cat shows emotional distress in response to the buzzer even though this CS does not elicit the avoidance CR.

Ablation of the septal region in rats renders an animal ferocious and hyperreactive to any stimuli (18). This finding is confirmed by two recent studies. Thomas *et al.* (215) reported, in addition, that these highly emotional rats showed a normal rate of learning a Lashley III maze, but they made significantly more errors and had longer time scores. The authors believed that these effects were caused by the excessive exploratory activities of the rat and not by sluggishness or disturbed neural mechanisms of learning. King (110) found that the operated rats learned a conditioned avoidance response faster than normal animals. Contradictory results of Harrison and co-workers (87, 125, 219) suggest that there are uncontrolled variables in these experiments. They found that septal lesions did not invariably induce hyperreactivity to stimuli. Such lesions abolished the CR of lever pressing to avoid a noxious noise but not the CR to the same noise for food reward.

Several studies have revived the interests of the akinetic mutism pro-

duced by lesions in the periaqueductal grey in cats (7, 106). Adametz & O'Leary (2) reported mutism, but not akinetism, after unilateral ablation of central grey. Skultety (203) attempted but failed to find the specific locus of this symptom in the periaqueductal grey. He also found that such lesions did not disturb gastric motility, insulin tolerance, water metabolism, or temperature regulation of cats. Mutism was also reported by Melzack *et al.* (132) in four out of five cats with central grey lesions. These authors studied the escape response of cats to the pain stimuli, pin prick and heat. Lesions in central grey or spinothalamic tract resulted in decreased escape response, attributable, presumably, to decreased pain sensitivity; lesions in the central tegmental fasciculus, on the other hand, caused hyperreactivity to pain stimulus. Combined lesion of central grey and central tegmental fasciculus did not change the cat's threshold of escape response. An antagonistic system in the central midbrain for pain transmission is indicated.

The behavioral effects of frontal lesions in monkeys are well known. Attempts have been made to produce these effects by direct lesion in the caudate nucleus, since anatomical studies suggest that the caudate nucleus sends fibers to the prefrontal cortex. Dean & Davis (42), as well as the earlier studies by Rosvold and co-workers (188, 189), reported hyperactivity and impairment of delayed response performance following bilateral caudatal lesions in monkeys. That these two symptoms were not correlated indicates separate neural mechanisms. Thompson (216) studied frontal decortication plus caudatal lesion in cats, and found that such lesions caused the animal to be disoriented and unresponsive, and to assume bizarre postures. Large caudatal lesions decreased the resistance to extinction of an avoidance CR to clicks established after frontal removal. Massive lesions (70 to 90 per cent) of the caudate nucleus and the frontal cortex prevented the cat from reacquiring the conditioned response.

*Corpus callosum.*—Studies from Sperry's laboratory have analyzed the role of the corpus callosum in interhemispheric integration (143, 147, 208). After sagittal section of the optic chiasma and corpus callosum, cats and monkeys did not retain a visual habit learned with one eye when tested with the second, untrained eye. These results, plus the demonstrated failure of interocular transfer in kittens and chimpanzees reared under monocular stimulation (37, 181), indicate the importance of the corpus callosum for both the development and maintenance of interhemispheric communication. Myers (145) localized the critical region of the callosum for the interocular transfer at its caudal 25 per cent. He calculated that there were about  $2\frac{1}{2}$ - to  $3\frac{1}{2}$ -million fibers in this portion of the callosum. Myers (144) further found that the interhemispheric communication was less influential than direct sensory input in learning. A cat with chiasma sagittally sectioned transferred a visual habit readily from the trained to the untrained eye. However, when two conflicting habits were given, one to each eye, interocular transfer was absent. The interhemispheric communication was not



sufficient to differentiate the two conflicting habits within a hemisphere. Recently this type of experiment has been extended to somesthesia. Myers (56, 146) reported in two abstracts a lack of transfer of a learned somesthetic task from one hand to the other in monkeys and chimpanzees. Sperry (207) reported that ablation of one somato-sensory cortex in the callosum-sectioned cat prevented the animal from learning a somesthetic discrimination with the contralateral paw.

Two studies of animals with divided chiasma and callosum dealt with visuo-motor integration. Schrier & Sperry (197) found that such a cat learned visual form discriminations through one eye (the other eye was covered) equally well whether the homo- or the contralateral paw was used to respond. Thus, there may be direct control of each limb from the homolateral cortex, or there is a subcortical center controlling all limbs. Downer reported that a monkey with such lesions would always use the hand contralateral to the open eye to learn visual discriminations (54, 55). No data are available on the learning rates when the monkey was forced to use the hand either homo- or contralateral to the open eye. Downer also reported awkwardness and difficulty, which can be gradually improved through prolonged usage, for the monkeys in using the hand homolateral to the open eye.

Two Russian studies reported effects of partial section of callosum in CR experiments. Bianki (13) found that damage of the anterior part of the corpus callosum did not affect the conditioned salivary response in one dog. The CR in another dog was reduced 50 per cent after the surgery when the CS was light or touch, but only a slight degree when sound was the CS. A reduced alimentary CR to sound following partial lesion of the callosum in dogs was reported by Mosidze (140). This author also succeeded in obtaining salivary CR in one side of the tongue, using sound to one ear as the CS and acid to one side of the tongue as the US. Following partial section of the callosum, the CR of one side of the tongue was not evoked by either the homo- or the contralateral monaural CS. These CR's could be re-established within 41 to 74 days after the surgery. This study together with the results of Schrier & Sperry points to the complexity of interhemispheric organization. Corpus callosum is but one of several communication systems.

*Association areas.*—Recently, Pribram (171) proposed a hypothesis on the functions of the association cortex. He discussed this theory in detail and supplied an experimental illustration (170). The association areas are divided into two systems. One is the parietotemporal cortex and its afferent inflow. This posterior intrinsic system is said to be differentiative in function, dealing with the systematic transformation of the input. A monkey with bilateral lesions in the posterior association cortex showed more deficient sampling behavior in a multiple-object problem than normal animals. The other intrinsic system, the frontal cortex plus the midline thalamic and limbic system, serves an intentional function as the site for the transformation of the outcome of actions. The effect of prefrontal ablation was to

increase the preservation of choosing the wrong object in the multiple-object experiment. A theory of brain functions is useful if it provides insight into the method of analysis (89); it should not be a mere restatement of experimental data, or a working assumption for the guidance of research, for such assumptions are implicit in all analytic experiments.

Effects of bilateral prefrontal ablation on the animal's activity and delayed response performance continue to receive experimental analysis. French (71, 72) obtained hyperactivity after area-9 lesions in monkeys that showed decreased frequency and duration of lever-pressing in an operant-conditioning situation. Mishkin & Weiskrantz (133) found an impairment of visual discrimination performance in prefrontal monkeys when a delay of 8 sec. was introduced between correct response and reward. If the intra-trial delay was introduced gradually, no such detrimental effect appeared. Prefrontal injury also impairs simple sensory discrimination (36). Habits based upon visual, somesthetic, and auditory cues (17, 66, 158, 230) were affected, though relearning was demonstrated in each case. Orbach & Fisher (158) have further claimed that bifrontal monkeys could be taught to perform effectively in a delayed-response problem. They concluded that individual monkeys depend upon different sensory cues to solve this type of problem.

A series of papers from Konorski's laboratory (25 to 28, 119) continues their work on the effects on CR in dogs of ablating both prefrontal areas. The operated animals could no longer avoid responding to a negative differential CS, or to an external inhibitory CS. Bruckowski reported that classical alimentary and instrumental salivary CR's were equally affected in the same operated dog; these inhibitory CR's could be re-established at about the same rate. He found also that the operated dog showed augmented salivary UR's during the intertrial interval as well as to food. Konorski's group proposed an inhibitory function normally present in the prefrontal cortex, but this hypothesis has not been completely accepted. A discussion of this controversy will be included in the Montevideo Symposium on Brain Mechanisms and Learning.

Bilateral ablation of the middle and inferior temporal gyri in monkeys results in a slower rate of learning and lack of retention of visual discriminations. Most investigators consider this postoperative deficit as specific to vision but not as resulting from field defects or disturbance of visual acuity. However, a different interpretation has been recently proposed by Pasik *et al.* (159). They found that monkeys with temporal lesions learned form discrimination at a normal rate provided the visual stimuli were large in size. Also, some of these monkeys showed difficulty in learning a tactual discrimination. The authors concluded that this impairment could be caused by a primary visual defect and a disturbance of general learning capacity. The experiment of Wilson & Mishkin (231) designed to separate visual-sensory and visual-learning defects does not support the interpretation of

Pasik *et al.* Wilson & Mishkin found that monkeys with temporal lesions were deficient in acquiring object learning set and form discrimination. Monkeys with lateral occipital cortical removals showed difficulty in learning string-pattern problems and colored-pattern discrimination. The former tests deal primarily with visual learning functions, and the latter with visual acuity functions. In another study, the critical flicker frequency was found to be lower in monkeys with temporal or lateral occipital lesions as compared with that of normal controls or monkeys with frontal lesions. Because of the continuous improvement of the critical flicker frequency during testing period, Mishkin & Weiskrantz (134) considered this result not contrary to the localization of learning and acuity functions in the temporal and occipital cortices, respectively. Ettlinger's (64) results also support this hypothesis. His monkeys with temporal ablations showed no deficit in brightness discrimination but had difficulty in learning visual form problems.

The anatomical pathways responsible for the interaction of visual and temporal areas are normally confined to the same hemisphere. Ettlinger (65) prepared monkeys with one optic tract lesion plus ablation of either homo- or contralateral temporal cortex. Only the latter type of lesion affected visual discrimination learning. Retention of visual discrimination was not disturbed by undercutting, but was abolished by crosshatching the temporal cortex. It seems that cortico-cortical connections between the temporal and occipital areas are essentially involved in visual discrimination (35). This is the only demonstrated instance of the functional significance of the intercortical transmission.

*Sensory systems.*—Electrophysiological studies have revealed hitherto unexpected complexity in the organization of the sensory systems. Sensory evoked potentials can be obtained in the association areas in cat and monkey under chloralose anesthesia or in curarized preparations (3, 30, 31, 120, 217). These potentials persist even when the primary sensory areas are completely destroyed (217). The related discussion of Bishop (15) on the implications of different fiber size for the manner of afferent innervation of the cortex provides a new look into the functions of the sensory systems. Microelectrode recordings in both anesthetized and waking animals is another method yielding valuable information (62, 63, 73, 105, 186, 229). Examples are the receptive field of a neuron in the striate cortex (96, 98), the spectrum sensitivity of lateral geniculate units (48), the units for acoustic attention (97), and the organization of units receiving different sense modalities into vertical columns in the sensory cortex of monkey (141, 142, 167, 168). Another important aspect in the elucidation of sensory mechanisms is the demonstrated corticofugal control of sensory input at both the peripheral and central stations (4, 46, 47, 190).

Blake (16) reported defects in visual recognition and visual discrimination following bilateral ablation of superior colliculi in cats. These cats

failed to retain, but relearned, brightness discrimination. One operated cat failed to acquire, and another failed to relearn, a form discrimination. Orbach (155, 156) demonstrated disturbance of the retention of a maze habit following bilateral occipital lesions in blind monkeys, but unilateral ablation had no such effect. Orbach discussed these results in relation to Lashley's contention of nonvisual functions of the visual areas, and to the problem of mass action.

Neff and his co-workers continued their studies on the role of auditory cortex in the auditory learning problem. Goldberg *et al.* (77, 78) reported in two abstracts that cats with combined lesions in auditory areas I, II, and Ep, somesthetic area II, insular-temporal cortex, and part of suprasylvian gyrus relearn frequency discrimination. Cats could not learn an auditory pattern problem after lesions in the insular-temporal cortex, a region not usually included in the auditory receiving areas. Recent physiological and histological studies indicate, however, that the insular-temporal cortex receives some auditory afferent fibers (45, 49). Riss (182) reported difficulty of sound localization in cats with combined lesions in auditory areas I, II, and Ep.

The effect in cats of ablating somato-sensory cortex in infancy on the learning of a roughness discrimination at maturity was reported by Benjamin & Thompson (12). These animals showed much less detrimental effect than adult cats who suffered the same lesion. Both their learning rate and their differential threshold for roughness are slightly higher than those of normal cats. This cortical compensatory action is reminiscent of that evidenced by lack of motor disturbances in monkeys when their motor areas were removed in infancy (107), and by residual pattern vision in rats when their visual areas were removed in infancy (220).

Kruger & Porter (115) and Orbach & Chow (157) reported that monkeys showed difficulty in relearning somesthetic discriminations following lesions in somatic area I, or somatic areas I and II, or somesthetic area I and precentral motor area. No such effect was apparent with lesions in somatic area II or the precentral motor area alone. Lesions in somatic areas I and II abolished an instrumental conditioned reflex in dogs. These CR's reappeared within two to three months after surgery without further training (210).

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## VISUAL SENSITIVITY<sup>1,2</sup>

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### PUPILLARY AND ACCOMMODATIVE MECHANISMS

Stark & Baker (1) and Stark (2) add to and summarize some of the quantitative data on the pupil reflex. They have applied the techniques of linear servo-analytic theory to these reflex mechanisms and have shown that the pupil servo system is quite stable, has low gain and a frequency spectrum with the major components around one cycle per second and an attenuation of about 18 db per octave for frequencies above 1.5 c.p.s. The system shows a large phase shift but gains stability from the low gain, which at low frequencies is of the order of .16. They have increased the gain of the system experimentally by focusing a small spot of light at the edge of the pupil so that small changes in pupil size lead to large changes in the retinal illumination. This induces oscillations in pupil diameter and permits additional analyses of the frequency characteristics of the system. Their analysis shows that the 180° phase cross-over frequency is approximately 1.4 c.p.s. Several pharmacological procedures involving the application of autonomic drugs were used to illustrate the linkage between the frequency of the high-gain oscillatory system and the 180° phase cross-over frequency point.

Stark, Campbell & Atwood (3) have studied quantitatively the small continuous fluctuations in the area of the pupil observed even when the eye is illuminated with a steady source. Stark (2) presents the autocorrelation function and the power spectrum of these time functions. These analyses show that the major components of these oscillations are in the frequency range from .05 to .3 c.p.s.

Lowenstein & Loewenfeld (4) have studied the pupil response to near threshold stimuli (defined in terms of the pupil response). The threshold for eliciting a pupil response is about one logarithm unit above the normal visual threshold. The pupil response to these weak stimuli is characteristically of small magnitude, short duration, and of long latency. They have shown that there is a systematic decrease in latency with increasing intensity. In the low-intensity range an increase in the light intensity causes a greater change in contraction than a brighter stimulus of shorter duration.

The interest in the frequency characteristics of visual system reflexes has been extended to the accommodative system. Campbell, Robson & West-

<sup>1</sup> The survey of the literature pertaining to this review was concluded in April, 1960.

<sup>2</sup> Abbreviations used in this chapter include: ERG (electroretinogram); CFF (critical flicker frequency).

heimer (5) performed a spectral analysis of the fluctuations of accommodation when the subject is viewing a fixed field. When the field is viewed through a small pupil yielding a large depth of field, the major frequency components are below .5 c.p.s. When the field is viewed through a larger pupil (7 mm.) the frequency analysis shows a large component at approximately 2 c.p.s. They were able to drive the reflex system if the subject was presented with a stimulus that was oscillating over a range of .75 diopters; in such a case the peak region of the frequency spectrum changed.

Campbell & Westheimer (6) studied accommodative responses of the human eye using an optical system that permitted changes in focus without affecting the size or the illuminance of the retinal image. Fixed offsets of focus were induced by the experimenter, and the subject then made a readjustment to achieve maximum sharpness. All subjects learned to make settings rapidly and in the correct direction. When the task was changed from one of adjusting the focus of a white-light target to one of adjusting a monochromatic green target, some subjects immediately made correct adjustments, other subjects required training to relearn the task. This finding suggests that some subjects may have been using chromatic aberration as the cue for the direction of off-focus. With a reduction of pupil size which would influence spherical aberration, in addition to chromatic aberration, the subjects never reached the point of zero errors in their directional adjustments.

One result of relevance to a number of experiments on cues to depth and the problems of the perceptual constancies occurred when the subject was presented with an instantaneous change-over from a stimulus requiring one level of accommodation to a second stimulus, of the same shape but of different size, requiring a different level of accommodation. When the larger target was presented, the subject increased the accommodative level, even if the larger target had been made optically farther than the smaller target. The apparent-size cue seemed to override the other cues in determining the accommodative response.

#### ABSORPTION MECHANISMS

Over 20 years ago Wald showed that most of the vitamin A released upon bleaching of rhodopsin leaves the retina during light adaptation and returns to the retina during dark adaptation. Hubbard & Colman (7) have now shown that the total vitamin A in the eye (including the vitamin A aldehyde, retinene, bound in rhodopsin) remains essentially the same in light and in darkness. They consider that in light adaptation there is a flow of vitamin A from the retina to the pigment layers and that this process is reversed in the dark. The proportion of 11-*cis* vitamin A varies with light and dark adaptation, making up about 10 per cent of all vitamin A in the light and about 25 per cent in the dark.

Arden (8) has suggested that the frog retina contains a narrow band

photopigment with a peak adsorption at about 535 m $\mu$ , in addition to the more typical rhodopsin with a peak at about 502 m $\mu$ . Dartnall (9) has been unable to find evidence for the existence of this "535" pigment and feels that the observations of Arden can be accounted for on other bases.

Although the question of the presence of a photochemical process as the initial stage of the visual system has become a matter of definition, the question of the way in which the photochemical data should be incorporated into the formulation of the problems of visual sensitivity has undergone repeated scrutiny in the last decade and will continue to be an important focal point for research. One reason for this re-evaluation is to be found in the work of several British experimenters, notably Rushton (10), Rushton & Cohen (11), and Weale (12) and their associates. By exploiting an ophthalmological procedure of measuring the light reflected from the retina, they were able to follow the changes in concentration of absorbing materials in the retina as a function of time in the light or dark and to relate these changes to such measurements as the change in sensitivity during light and dark adaptation. This experimental procedure continues to be a powerful technique for investigating *in vivo* changes in retinal photopigments. In the last year Weale (13) has reported on an apparatus that permits more rapid measurements of absorption spectra. Using this improved reflection technique, Weale measured the foveal difference spectra of two normal subjects. The results show two major peaks, one at 550 m $\mu$ , a second at 600 m $\mu$ , and provide evidence for the influence of a product of bleaching having a major negative peak at the short wavelengths (approximately 450 m $\mu$ ). The regeneration curves presented agree substantially with the early work of Rushton. The results are interpreted as indicating that at least two photopigments are being bleached, but detailed quantitative conclusions are limited because of the possible involvement of the products of bleaching.

Denton (14) has extended the analysis of the organization of the visual sense cell by studying the orientation of absorbing molecules in the retina. Working with the all-rod eye of deep-sea fishes, he has shown that the difference in the density of the prepared isolated retina for light polarized across the axes of the rods and light polarized along the axes indicates that there is a high degree of molecular organization. These measurements and other observations by Denton suggest that the pattern of dichroism changes during bleaching and that the products of bleaching lie parallel to the axes of the rods.

These results on the structural organization of the sensory system have relevance for the general question of how detailed description of stimulus variables must be in the study of visual sensitivity. The way in which such details of structure and orientation may enter into psychophysical determinations of visual sensitivity is already exemplified in the now classic studies by Stiles & Crawford (15) on the directional effects in visual stim-



ulation. O'Brien (16) has demonstrated that results similar to the psychophysical measurements of Stiles & Crawford can be obtained with a physical model of a receptor, and he suggested that the cone shape acts to concentrate the energy from the larger inner segments into the smaller outer segments of the sense cells. In the past year Enoch (17) has continued this kind of physical analysis using a polystyrene model of a retinal cone and irradiating it with a microwave source of varying wavelength. He has shown that the amount of radiant flux absorbed as a function of the angle of incidence of the input exhibits marked directional sensitivity and that the function relating absorbed flux and angle of incidence of the input flux is, itself, a function of the wavelength of the radiant source. The more oblique the incidence, the more the longer wavelengths will be emphasized. In an attempt to answer the question whether such alignment problems are the basis of certain visual defects, Enoch (18) studied a number of amblyopes, using several procedures designed to get some index of the geometrical organization of the retina. The argument gets a little unwieldy because of the many consequences of misalignment; however, the results clearly indicate the importance of additional research and theoretical analysis exploiting these possibilities.

#### THE RETINAL IMAGE, EYE MOVEMENTS, AND VISUAL SENSITIVITY

DeMott (19) has studied the way in which the eye optically transforms the light pattern from external sources in forming an image on the retina. Most of the experimental measurements were made on excised steer eyes, although some measurements are reported on the cat eye with blood supply intact. Post-mortem changes have been traced from 38 minutes to 120 minutes, and the results are consistent over this period. Data are presented showing the relation between illuminance and the position along the retinal image of a black line on a light field. The general shape of the light distributions in the image follows qualitatively the form expected from a diffraction pattern and chromatic aberration analysis of this kind of stimulus material. However, the most striking thing about the quantitative details of these measurements is the marked numerical discrepancy between the measurements and those expected from a strictly geometrical optical analysis. The gradients of illumination are noticeably less steep than those expected from the optical analysis. DeMott feels that a large fraction of the blurring of the image must be attributed to scattered light. Since the results of Hecht & Mintz (20) and Hecht, Ross & Mueller (21) [see also (96) below] indicate that the psychophysical threshold for the type of stimulus figure used by DeMott lies below .5 sec. of visual angle, he has extrapolated his data to such a threshold figure and estimates that it involves a contrast of the order of .1 percent. He then points out that this is not an unrealistic figure in the light of other psychophysical data on contrast sensitivity.

Ogle (22) has studied the blurring of the retinal image by measuring contrast sensitivity at the fovea using a point source and stages of image focus. The greater the blurredness of the image, the greater the intensity required for the source to be seen against the 12 m.L. background. An intensity change of a factor of 10 is required for an image one diopter out of focus to be detected. A stimulus two diopters out of focus required an increase in intensity of about 25 times.

Westheimer (23) has analyzed the effect of the size of pupil on the distribution of light in the image formed when a Maxwellian view is used. The development shows that the sharpness of the image is increased by increasing the number of diffraction rings that are admitted to the eye, and Westheimer has presented equations for calculating the distributions as they would be modified by the Stiles-Crawford effect.

The techniques developed independently by Ditchburn & Ginsborg (24) in England and by Riggs *et al.* (25) in this country for stabilizing the retinal image against the small involuntary nystagmic movements continue to provide valuable data for analyzing the phenomena of visual sensitivity. The early techniques gave stabilization for rotations around a vertical axis and, to a considerable extent, around the horizontal axis. In the past year Clowes & Ditchburn (26) have described a technique that, in principle, gives complete stabilization for rotations and translations at one point in the field. It also provides stabilization for the illumination received by the eye. The residual movement after stabilization is of the order of .003 minutes of arc, or less than 1 per cent of the intercone distance.

Ditchburn & Fender (27) investigated the effect of experimentally inserting various kinds and magnitudes of retinal image motion through a system that eliminates or minimizes the intrinsic motions due to eye movements. The results indicated that a displacement of the order of one minute of visual angle would restore the visibility of a target that has disappeared because of stabilization. Krauskopf (28) reported similar results. The past year has provided additional and more detailed data on the visibility of targets that have specifiable rates and magnitudes of motion on the retina. Riggs & Tulunay (29) used a technique that altered the optical path length of the projection system having a mirror on a contact lens as one component. The measure of visibility employed was the percentage of time the target was seen. The target was minimally visible for errors of stabilization near zero, i.e., minimum image movement; target visibility increased with increasing errors of stabilization, both positive and negative.

Ditchburn, Fender & Mayne (30) studied visibility as a function of the amplitude of the movement of an oscillating target. Their results show an increase in visibility as a function of the imposed amplitude of oscillation up to a magnitude of approximately 25 minutes of arc. Above this amplitude the function showed a small decline in visibility. When they studied the visibility of short step-like changes in position, called "flicks," they

also found a function reaching a peak and then exhibiting a decline in visibility. For all frequencies tested, small amplitude tremors (less than .3 minutes of arc) were worse (in terms of visibility) than was the stabilized image. Above .3 minutes of arc there is a steep rise in visibility, and the visibility stays high except at high frequencies (above about 15 c.p.s.) where there is a decrease in visibility with increasing amplitude. The results show that slow drifts of the image restore visibility only when they are as large as the largest normal drift movements. It is interesting to note that many of the components of the normal eye movements fall within the depressed region below .3 minutes of arc.

Krauskopf & Riggs (31) report an experiment on the interocular transfer of stabilized stimuli presented to corresponding points in the two eyes. In 1921 Dunlap (32) reported that if a very dim spot were presented to one eye, the target eventually disappeared. If the same spot was then presented to the other eye in the corresponding position, it could not be seen. Krauskopf & Riggs have investigated this phenomenon by using an unstabilized bright annulus and fixation point to each eye and a stabilized disk and stimulus figure to each eye. In the pre-exposure period of 30 seconds the figure was located to the left or the right of the fixation point in the experimental and control conditions, respectively. The test field was a bar to the left of the fixation point in the right eye and was presented in a test period of 30 seconds immediately following the pre-exposure period. They found that the visibility of a stabilized figure depended on whether it fell on corresponding points, when referred to the pre-exposure figure in the other eye. It was seen a smaller fraction of the time, if it fell on corresponding points. The effect was small, on the order of 5 to 10 per cent, but was statistically significant.

Nachmias (33) has measured the two-dimensional motions of the retinal image during monocular fixation and reports the relative frequencies of drifts and saccades as a function of the direction of the motion. He noted marked variations in these directional movements, e.g., for one subject 63 per cent of all saccades were in the region between  $50^\circ$  and  $90^\circ$ . These directional functions were different for each subject.

Smith (34) has described a photoelectric technique for measuring eye movements. The technique is based on the difference between the iris and the sclera in reflectance. It utilizes this difference by taking a slit reflection 1 mm. wide and 1 cm. long from the iris-sclera region and feeding this reflected light into a photomultiplier tube. As the eye moves, more or less of the image of the iris falls on the slit and, thus, the input to the photomultiplier tube changes.

Hyde (35) has presented quantitative data on human voluntary eye movements in the horizontal dimension. The eye movements measured were the voluntary fixation movements to two targets placed at varying distances from one another. As the eye left one fixation point, the speed of move-

ment increased to a maximum and then slowed down as it approached the second fixation point. The maximum velocity increases with the distance of the total movement to a much greater extent than does the acceleration or the average velocity. The travel time increases linearly with the total distance of the movement and the mean velocity for making various magnitudes of fixation movements changes only slightly with the increase in size of the movements.

Crawford (36) has shown that the saccadic eye movements in response to moving targets may be complex when the targets are moving at high velocity. The interval between the first and second saccades observed may be on the order of 50 to 70 msec. Since this is of the same magnitude as the time for an afferent volley to reach the cortex, it is felt that a reassessment of the mechanisms of oculomotor activity is needed.

An important contribution to our understanding of the mediating mechanisms in eye movements has been made by Hyde & Eason (37). They have shown that stimulation of a particular point in the superior colliculus, in the reticular substance of the medulla, and in the dorsal midbrain tegmentum yielded eye movements that tended to bring the eye to the same final position; the eye tended to maintain this position as long as the stimulus persisted. There was frequent variability in the exact path taken to reach the position. Measurements of the eye movements showed that the velocity reached its peak within .25 seconds after the onset of the stimulus and then slowed exponentially as the eye approached its final site. The total time to reach the fixation site was approximately constant regardless of the length of the fixation movement. These results are quite consistent with Hyde's measurements of human voluntary eye movements (35). Some interesting "off" effects were noted.

Hebbard & Marg (38) measured the small involuntary nystagmic movements in cats by suturing a mirror to the cornea. The small tremors varied in frequency from 35 to 63 c.p.s. and averaged about 50 c.p.s.; they varied in amplitude from about 4 to 52 seconds of arc and averaged about 22 seconds of arc.

#### THE INITIATION OF ACTIVITY IN VISUAL SYSTEM

*Some results from microelectrode recording.*—If one inserts a micro-pipette into a single ommatidium of the compound eye, one can record two types of electrical changes. One of these is a slow positive potential, presumably representing a depolarization of the receptor cell; the second is the more rapid impulsive activity typical of optic-nerve recording. Frequently, it is possible to obtain records in which both of these signals are present, and such records permit some study of the interrelations of these two electrical changes. Hartline and his associates (39) and MacNichol (40) have reported an approximately linear relation between the logarithm of the intensity of the stimulating light and the magnitude of the slow po-

tential, and a linear relation between the magnitude of the slow potential and the frequency of nerve-impulse activity. These authors also report that one can alter the frequency of spontaneous discharge by applying a potential that would cause an increase or decrease in the membrane potential; positive potentials increase the frequency of discharge, negative potentials decrease the frequency. An approximately linear relation exists between the applied current and the change in frequency.

Fuortes (41) has confirmed many of these findings in the past year and has offered a number of additional analyses of the data. For example, he has used measurements of the type mentioned above to compute the membrane resistance. He concluded that, during illumination, there is a decrease in membrane resistance and that the change in membrane conductance may be the cause of the generator potential.

On the basis of some results obtained with microelectrode recording in the insect eye, Naka & Kuwabara (42, 43) feel that the electroretinogram can be analyzed into two monophasic potentials, and they present an analysis similar to those offered by a number of previous investigators (44, 45, 46). These authors also report the sequence of potentials and the changes in waveform of the slow potentials that result from inserting a micro-pipette into the ommatidium of the compound eye with the axis of the electrode coincident with the axis of the eye.

Svaetichin's early reports on microelectrode recording in the fish retina have stimulated a number of experiments in the vertebrate retina. Part of the difficulty in properly evaluating the nature of these potentials stems from a problem present in all experiments using microelectrodes—the question of accurately stating where the tip of the electrode is when the records are taken. Progress has been made on this problem, although present techniques have still not achieved the resolution desired. Oikawa, Ogawa & Motokawa (47) reported staining points as small as  $36\ \mu$ . On the basis of the work of a number of investigators, it is generally agreed that the Svaetichin potentials represent activity of cells at the level of second order neurons. This conclusion is based partly on the results of microelectrode tip marking procedures and on experimental observations of the nature of the potentials, e.g., that they are a function of the area stimulated. Tomita *et al.* (48) have shown that these slow potentials depend on the area stimulated; the smaller the area of the stimulus, the smaller the potentials recorded. They observed no sharp change in the response as a border moved across the recording site. They also report that a variety of wavelength response curves can be recorded depending on the region of the retina from which the recordings are made. Yamashita (49) reports on the spread of these local potentials. The response is greatest when the stimulus is at the recording site, and measurements taken when a small cut was made near the recording electrode indicated that this spread was not due to stray light.

Motokawa *et al.* (50) have studied the phenomenon of flicker while re-

cording these slow potentials. These potentials, which generally give a rectangular pulse having the duration of the light stimulus, exhibit a ripple when the light is flickered; the prominence of the ripple is a function of the flicker rate. These authors measured the analogue of the critical flicker frequency of a psychophysical experiment. For the *A* elements, whose polarity depends on the wavelength, this physiological CFF was about 72 c.p.s. For the *N* elements, whose change in potential is always in the negative direction, regardless of wavelength, this CFF was about 48 c.p.s. For any given frequency the prominence of the ripple was always greater in the *A* elements than in the *N* elements. The relation of the CFF to the intensity of the flickering light was similar to that obtained in the human psychophysical experiment.

*The electroretinogram.*—Armington & Crampton (51) found that the latencies of evoked potentials recorded from the optic tectum could either precede or follow the *b*-wave of the ERG and that these two response measures can show different spectral sensitivity curves, even when recorded simultaneously. Crampton & Boggs (52) have made a detailed study of the latencies of the ERG and the evoked potentials in the optic tectum. The analyses of the latencies of the *a*-wave, the *b*-wave, and the tectal response show, in general, that there is a relatively large difference between the *a*-wave and the *b*-wave (on the order of 10 msec.), but little difference between the *b*-wave and the tectal response (on the order of 1 msec.). The influence of the level of adaptation on the latency of the retinal response is small and varies with the test intensity. One of the interesting features of the results is the variability reported. Some birds showed a Purkinje shift at the tectum, but not at the level of the ERG; some showed no increase in sensitivity during dark adaptation in the tectum, but showed such changes in the ERG.

Forbes *et al.* (53) find that the spectral sensitivity curves for several types of all-cone retinas exhibit the property now commonly attributed to the human spectral sensitivity curve, i.e., a major peak and several shoulders, or "bumps," at shorter and longer wavelengths. They feel their results suggest that multiple types of cones are involved in the visual process. Dodt & Walther (54) report on the ERGs of two types of nocturnal Geckos. The spectral curves, defined for a constant height of the *b*-wave, peaked at around 530 m $\mu$ , which is in rough agreement with the research on relevant photopigments; however, the sensitivity curves were broader than any absorption curves of photopigments so far extracted. The suggestion is that there is more than a single photopigment involved. They also note that these species have low thresholds, on the order of one-tenth that obtained from the cat when the same kind of recording and stimulating procedures is used. Tansley (55) has filled a gap in our information about the retinal structure of the species studied by Dodt & Walther, and others. In general, the description of the retina follows that reported for other



species of nocturnal Geckos. The large visual cells seem to be double, i.e., they have two outer segments and two nuclei for each inner segment. It is Tansley's view that the number of true visual cell nuclei and the number of ganglion cells are about equal. Dodt & Walther (54), using direct and diasclical stimulation, find that the spectral sensitivity curves obtained on the rabbit are different. Their results provide evidence of the influences of the Soret band of hemoglobin absorption in the case of diasclical stimulation.

A number of studies this year were devoted to investigation of changes in the human electroretinogram. Biersdorf & Armington (56) have shown that the changes in the human ERG during adaptation are not large. The major changes occur at high levels of illumination, where there is a decreased ERG resulting from steady-state light adaptation. The long latency components seem to be affected by adaptation to a greater extent than the short latency components. Armington (57) has further shown that the dependence of the amplitude of both the negative and positive waves of the ERG on intensity varies with the wavelength of the test stimulus and the wavelength of the stimulus to which the eye is adapted. The slope of the line relating amplitude and the logarithm of stimulus intensity is steeper for the longer adapting wavelengths and the shorter testing wavelengths. The spectral sensitivity curves obtained, with a constant positive deflection as the criterion response, showed a maximum around 500 m $\mu$  for all of the wavelength adaptation conditions. The effects of selective adaptation are shown mainly at the red end of the spectrum. Because of the changes of slope in the function relating amplitude of response and intensity of the test stimulus, the spectral sensitivity curves will exhibit variations due to the amplitude level selected as the criterion response.

Rendahl (58) discusses some of the changes in the waveform that are produced by selective preadaptation to red and to green light and uses these changes in waveform found for the normal eye to evaluate certain characteristics of protonopia and deuteronomia.

Burian & Spivey (59) have studied the effect of double flashes and repetitive stimulation on the human ERG. They obtained data on the size of the *a*- and *b*-waves for the two flashes as function of the intensity of the test flashes. For low intensities the *a*- and *b*-waves for the second flash were larger than those for the first flash, a relation which was reversed for high test intensities. The lower the flash intensity, the longer the separation required in order for the two flashes to be resolved. At high intensities these authors observed a resolution of about 40 msec.; for the low intensities this figure was about 100 msec.

*Additional studies of electrical activity in the visual system.*—Gaze (60) and Maturana *et al.* (61; see also 62) have provided us with the physiological equivalent of the behavioral experiment by Sperry (63) on the orderly regeneration of severed optic nerves in amphibians. Gaze recorded potentials from the optic lobes of the normal animal and mapped the pro-

jection of the visual field onto the lobes. The optic nerves were sectioned in the tadpole stage and allowed to regenerate. After metamorphosis, he found that the normal mapping had been restored. In another experiment the eye was rotated, following a procedure used by Sperry. After regeneration the map of the visual field as measured by potentials in the optic lobe had been similarly rotated. Maturana *et al.* have given a more detailed description of the organization of the frog's tectum. They have shown that the optic-nerve fibers end in a systematic way both along the surface of the tectum and in depth along the superficial neuropil. The several layers of this neuropil each map the retina, and the different layers seem to have a specificity with respect to the "on," "on-off," and "off" systems. As one progresses from the most superficial to the deepest layer of the neuropil, one encounters first the "on" response system, and last the "off" system. These authors identify a fourth category of fibers called "convexity detectors" which they place between the "on" and "on-off" layers, although they report that this region is less sharply localized than the others.

Burt & Catton (64) have studied the timing of neural events at different points in the visual pathways in the insect. They found relatively long delays in the optic lobe region and report that both ipsilateral and contralateral pathways run from the optic lobe to the nerve cord. Ochs (65), using electric stimulation of the optic nerve and direct stimulation of the cortex, has studied some of the properties of evoked potentials in the visual cortex. Koella (66) reports a number of quantitative details of evoked potentials in the cerebellum as a result of light stimulation. Increasing the intensity or the duration of the stimulus decreased the latency of the evoked potentials, and there was an approximate interchangeability of intensity and exposure time. It was possible to measure such evoked potentials in the cerebellum at intensities that would yield no measurable ERG.

Jung (67) reviewed some of the research of the Freiburg group and pointed out a few of the many parallels between the human discrimination data and the results of single fiber recording in the cat cortex. The logarithmic nature of the dependence of cortical neuronal activity on the intensity of the light, the quantitative dependence of flicker fusion on intensity, and the brightness enhancement effect in flicker are among the parallels studied by this laboratory. In the past year Grusser & Grusser-Cornehl (68) and Grusser, Grusser-Cornehl & Saur (69) demonstrated some of the interactions of light stimuli and stimulation of the vestibular system on single neurone activity in the visual cortex. This group had previously shown that the maximum CFF exhibited by cortical neurones was influenced by stimulation of the nonspecific thalamic nuclei and the reticular formation.

#### SOME PHYSIOLOGICAL MECHANISMS OF PERCEPTION

The past few years have yielded a number of experiments that provide a substantial beginning in the physiological analysis of perception. Perhaps

the most complete and quantitative account of the kind of interactive systems operating in vision is to be found in the series of experiments by Hartline and Ratliff. The system of lateral inhibition described and experimentally studied by these authors can generate such phenomena as simultaneous contrast and many areal and distance effects. An additional report (70) this past year extends the experimental analysis and demonstrates that this inhibitory system also generates the equivalent of Mach bands. This set of experiments realistically illustrates the feasibility of a quantitative physiological attack on the problems of pattern vision.

Although the work on the vertebrate retina has not yet reached this stage of development, a complex set of excitatory and inhibitory interactions has been clearly outlined by the work of Hartline (71), Kuffler (72), Barlow (73), and others. The extensive set of observations of Hubel & Wiesel (74) in the last year has indicated some of the principles that underlie the complexities of the sensitivity of the vertebrate retina to patterned stimuli. Recording from single neurones in the striate cortex of the cat, they have mapped the receptive fields of a large number of cells. These receptive fields differ in a number of respects from the fields obtained from retinal ganglion cells. The main differences are seen in the spatial arrangement of the excitatory and inhibitory regions of the receptive field. The cells from which they recorded do not have the simple concentric arrangement so typical of retinal recording. The two most common patterns observed showed a center field, markedly elliptical, either excitatory or inhibitory, and flanked on both sides along the minor axis by approximately semi-circular fields of opposite action, i.e., either inhibitory or excitatory, respectively. Stimuli that would activate equally the excitatory and inhibitory areas of a neurone would produce no activity. Circular targets placed so as to cover both the excitatory and inhibitory fields represent one example of such stimuli; rectangular stimuli oriented along the minor axis of the central field were also typical targets in this category. If the rectangular target was rotated toward the major axis of the central field, a response was obtained; whether the response was an "on" or an "off" response depended on the nature of the central field. The responses obtained from stimuli of different orientation, or stimuli undergoing different translational movements, could be treated in terms of the basic data on the organization of the receptive fields.

Maturana *et al.* (61) and Lettvin *et al.* (62) have classed into four categories the fibers in the frog optic nerve and optic tectum: (a) sustained contrast detectors, (b) net convexity detectors, (c) moving edge detectors, and (d) net dimming detectors. Categories (a), (c), and (d) are respectively, the "on," "on-off," and "off" responses of earlier investigators. As mentioned above, these fibers are distinguishable in terms of the stimuli that will elicit activity in the fiber and in terms of their projections to the optic tectum. Category (b) is felt to be a separate type, although the authors state that there are gradations into (a) in terms of effective stimuli;

the projection layer for (b) is also less clearly marked. Because of these partial confusions accompanying category (b) and, particularly, because the stimulus operations obviously involve the spatial interactions so extensively studied by other investigators, a thorough experimental analysis would seem to be indicated before we adopt it as a type or a category.

The recent single-neurone recording in the lateral geniculate body and the optic cortex also seems to offer initial steps toward understanding some of the phenomena of binocular interaction and such related problems as stereoscopic vision. The crossing of fibers in the optic chiasma that provides fibers from both eyes to each geniculate body is such that the fibers from each eye terminate in separate cell layers. The anatomical studies of Hayhow (75) show that the interlaminar regions contain large cells that receive fibers from both eyes. This suggests that these regions may be involved in the integration of signals from the two eyes. Erulkar & Fillenz (76) have recorded from single units, in the geniculate area, which responded to light stimuli presented to either eye. Bishop, Burke & Davis (77) have also recorded from single cells in the lateral geniculate and suggest that there are three main categories of binocular interaction: a direct interaction involving short latencies of response; a delayed interaction with long latencies (in the range of 100 to 300 msec.); and an indirect interaction in which cells can be stimulated with stimuli to only one eye, but the firing pattern of these cells can be influenced by stimulation of the other eye. DeValois, Smith & Kitai (78) also recorded from single cells in the lateral geniculate nucleus and report that cells in layers 1, 4, and 6 responded to stimulation of the contralateral eye, and cells in layers 2, 3, and 5 responded to stimulation of the ipsilateral eye. They found no cells that could be stimulated through both eyes but did not test for other possible bilateral influences.

About 20 per cent of the neurones in the striate cortex, studied in detail by Hubel & Wiesel (74), could be driven by the two eyes independently. When they mapped the receptive fields of these fibers they found that these fields were roughly in homologous parts of the two retinas; they were similar in shape, in the orientation of their axes, and in the arrangements of the excitatory and inhibitory regions within the field. The summative and antagonistic phenomena of the excitatory and inhibitory areas observed for a single eye were found to hold for these fibers when binocular stimulation was employed.

#### SOME ADDITIONAL PSYCHOPHYSICAL RELATIONS

*Flicker discrimination.*—Lloyd & Landis (79) studied the role of the light-dark ratio in determining foveal flicker discrimination. There was a tendency for the smaller light-dark ratios to show a steeper rise of the critical flicker frequency with increasing intensity than was obtained with the larger ratios. For large light-dark ratios, and particularly with the 2° area, there was a tendency for the CFF-intensity curves to exhibit a sec-

ondary rise, after reaching a plateau at moderate-to-high intensities. Graham & Landis (80) report data on the relation of critical flicker frequency to intensity for four sizes of grating. If the critical flicker rate is plotted as a function of the number of lines per inch in the grating, the function passes through a minimum for all intensities, and the minimum seems to shift to the finer acuities with the lower luminances of flickering light. A problem studied in 1931 by Graham & Granit (82) has been reinvestigated by Luria (81). The earlier authors found no evidence of inhibition in the periphery, although inhibition was demonstrated in the fovea. Using two adjacent, semicircular, flickering fields of different intensity level, Luria found evidence of a contrast, or inhibitory, effect both in the fovea and in the periphery. Simonson (83) has offered data on fusion thresholds for flicker between two levels of luminance, neither of which was zero. There was a decrease in the critical flicker rate with increasing ambient illumination with a relatively sharp drop for ambient illuminations above about 75 per cent.

Gebhard & Mowbray (84) have continued their work on some of the relations between visual flicker and auditory flutter. Their earlier results had shown that the differential sensitivity for frequency was of the same order of magnitude for both the eye and the ear, although the functions relating the difference thresholds to the reference frequency were different for the two modalities. They now report that the difference limens (DL) for matching flicker to flutter are smaller than those for matching flutter to flicker; both of the DLs increase as the standard frequency increases. These results hold even if the procedure is one of successive matching, rather than simultaneous matching. In general, they found that the cross-modality discriminations were poor compared with the intramodality matching by a factor of about 10.

Brown & Forsyth (85) and Forsyth & Brown (86) have used an intramodality matching procedure to study an interesting variation of the standard flicker technique. They were interested in the case where the alternate "on-off" cycles of the flicker sequence were of different durations. They studied such a train of light pulses for the criterion of fusion and for the case where one of the pulse durations was to be adjusted so that the complex train matched the subjective flicker rate of a standard flicker stimulus. The standard flicker stimuli were set at 25, 28.6, and 33.3 c.p.s. They plot the data as flicker contours, showing the combinations of durations of the two light-pulse durations involved in the complex train of pulses that would yield the criterion match. For some values of the duration of one of the pulse components, as many as three values of the duration of the other pulse would yield a fixed apparent flicker rate. For example, when the standard flickering stimulus was set at 33.3 c.p.s. and the duration of one pulse component of the train was set at 27.5 msec., one observer made a flicker match at pulse durations (of the second component of the train) of 3, 13, and 32 msec. Levinson (87) has followed the lead of a number of

earlier workers and treats the Brown & Forsyth data, using the assumption that the subject detects one component of a Fourier analysis of the stimulus time function at an amplitude that is independent of other components below threshold. This use of a Fourier analysis of visual stimuli goes back, at least, to the papers of Ives, almost 40 years ago; Ives (88, 89) felt at that time that the fusion frequency could be described in terms of a Fourier analysis of the flickering stimulus. A decade later Cobb (90, 91) adopted a similar position, but it was another two decades before this position received re-emphasis in the work of de Lange (92). Operating on certain symmetry characteristics of the Fourier function, Forsyth (93) studied four different stimulus trains that have identical frequency spectra. Using trains of triple pulses of light, each having an equal dark time associated with it, he has studied the flicker contours associated with variations in the durations of the three light pulses. The equivalences in terms of the Fourier analysis are achieved by reversals of the light and dark periods and reversal of the positions of the second and third pulses in the triplet. The four trains of pulses give essentially similar flicker data.

*Some threshold measurements.*—Baker, Doran & Miller (94) have traced the rapid changes in threshold immediately following the change from one adapting luminance to another. They studied these changes in the fovea and parafovea at photopic levels and in the parafovea at mesopic and scotopic ranges of luminance. These changes in the difference threshold were studied for a number of magnitudes of preadapting levels and a number of magnitudes of change in luminance. The difference thresholds showed a very rapid drop immediately following the change in luminance, changing as much as 1 to 1.5 logarithm units within .25 seconds. Battersby & Wagman (95) have used a procedure similar to the early work of Crawford, and the later work of Baker & Boynton, for following the changes in threshold at various points in time with respect to a conditioning flash. In general their results are similar to those of earlier workers, except that the terminal rise in threshold at the end of the conditioning exposure, reported by Crawford & Baker, was not observed.

Ogilvie & Taylor (96) have extended the work of Hecht & Mintz (20) and of Hecht, Ross & Mueller (21) on the visibility of fine lines. They worked with two different lengths of line, with two different length-width ratios, and with squares of varying area. The threshold values of .5 sec. of arc for long lines and 10 to 20 sec. for squares are in general agreement with the results of Hecht, Ross & Mueller. In addition, Ogilvie & Taylor show a systematic decrease in the area (at threshold) as the angular width of the target is increased. For three subjects, lines vertically orientated were more visible than those obliquely oriented. For one subject the oblique orientation led to greater visibility. Sensitivity as a function of meridian position was then studied in two subjects. Weymouth (97) has examined the orientation results of Ogilvie & Taylor and several previous workers and interprets them in terms of the characteristics of the visual image.



Since the time of Gullstrand, a number of workers have emphasized that the visual image of a point source is likely to assume a number of forms, but a typical one is an eight-point form, with the vertical and horizontal components being the longest and brightest. Weymouth discusses how such an aberration would lead to maximum integration in the horizontal and vertical dimensions and how individual differences in such patterns could lead to the observations of Ogilvie & Taylor.

Kincaid, Blackwell & Kristofferson (98) have outlined a theory of the effects of target size and shape on the visibility of the target. The essential notions in this theory are those offered 20 years ago by Graham, Brown & Mote (99). The major differences seem to be the introduction of a random variable notation and the decision not to suggest any specific element-contribution distance function but to solve for this function using the discrimination data.

Marriott, Morris & Pirenne (100) report that the energy requirements at threshold for a light exposed for 15 sec. is equivalent to about 100 to 150 quanta/sec. The interpretation of the results for such long exposures is complicated by questions of integration time, multiple "looks," and a number of other important considerations. In a second paper (101) these same authors have extended the threshold energy measurements to physiological experiments by measuring thresholds for modifying activity in single neurones in the cat lateral geniculate body. A comparison of their results and the behavioral measurements of threshold reported by Bridgman & Smith (102) and by Gunter (103) indicates that the physiological thresholds obtained are about three times higher than the behavioral thresholds.

*Contrast phenomena.*—Alpern & David (104) have pointed out the failure to get complete additivity in simultaneous contrast and have related the effects of adding additional inhibiting fields in the contrast situation to the area problem in contrast. One of the most interesting results reported is the demonstration of a disinhibition, or "inhibition of inhibition," effect. The similarity of these results to those of Hartline & Ratliff is discussed, and the relation of both sets of results to the problem of Mach bands is indicated.

Helson & Rohles (105) describe and investigate an interesting phenomenon in brightness contrast that seems to be contrary to most of the observations on simultaneous contrast. Comparing a striated field that has light lines on a gray background with a field that has dark lines on the same background, subjects agree that the light lines make the gray appear brighter, the dark lines make the gray appear darker. Helson & Rohles studied this effect using line separations ranging from  $3' 25''$  to  $1^\circ 11'$ . For all of these widths, the contrast reversal is observed.

By assuming a Gaussian filter function to calculate the retinal distribution of intensity and by adopting Mach's notion of the perceptibility of contours in terms of the second derivative of intensity, O'Brien (106) has

treated the Wertheimer triangle figure that has a triangle included in one arm of a cross and an added triangle between two of the arms.

*The perception of depth and some stereoscopic phenomena.*—The effect of fixation conditions on a real-depth discrimination has been reported by Lit (107). The changes with luminance are approximately equivalent for the three conditions of fixation studied: (a) fixation on the stationary reference rod, (b) fixation on the adjustable rod, and (c) fixation on either. The lowest thresholds were obtained for condition (b), the highest for condition (a); condition (c) yielded intermediate values. Lit (108) has also studied depth discrimination thresholds as a function of the difference in retinal illuminance in the two eyes.

Jameson & Hurvich (109) reported on the manner in which several cues to distance contribute to distance discrimination. The three factors studied were (a) changes in parallax, (b) changes in retinal image size, and (c) changes relating to accommodative mechanisms and image quality. They investigated conditions in which these cues operated singly, in pairs, and all these simultaneously. The function relating error in distance setting and observation distance was found to be different for each condition. The data were interpreted as indicating that, where multiple cues were available, sensitivity was the sum of the sensitivities to the component cues.

An interesting phenomenon occurs in stereoscopic vision when the near and far object and the fixation point all fall in the median plane. It is possible in this case to adjust the position of the fixation point so that the crossed images of the near object occupy the same lateral positions as do the uncrossed images of the far object. Zajac (110) studied this situation, described the geometry involved, and presented solutions for a number of the relevant variables.

The effect of target velocity on the magnitude of the Pulfrich phenomenon has been investigated by Lit (111). The general finding is that over a large range of velocities the general view of the Pulfrich phenomenon (as the production of an apparent disparity due to variations in the latent period with different stimulus intensities) seems to hold. The computed latency differences remain essentially unchanged as target velocity is increased. Lit (112) has also reported that the thickness of the target has no systematic effect on the Pulfrich phenomenon.

*Studies of apparent size.*—Heinemann, Tulving & Nachmias (113) have investigated the role of a variety of oculomotor adjustments, such as convergence, accommodation, pupil size, etc., in judgments of apparent size. They conclude that convergence is a sufficient condition for getting changes in apparent size and that pupil diameter and accommodative changes are not necessary. Jenkin & Hyman (114) have supplemented the earlier observations of Gilinsky (115) on the effect of attitude on size matches, with a factor analysis of results obtained using analytic and objective sets. Jenkin (116) has confirmed his earlier report that apparent size increases with

increasing distance. Since, in the data reported, he shows that the mean of the readings falls within .05 inches of the mean of the stimulus range offered to the subjects, it would be interesting to confirm this observation in an experiment where the subjects had a greater range or a more variable range of stimuli presented to them. Crookes (117) discusses certain size-constancy effects and the question of how such effects might be applied to the phenomenon of afterimages. He draws certain distinctions between the analysis of the perception of real objects and the procedures and analyses that are available for talking about afterimages.

*Pattern and shape discrimination.*—Green, Wolf & White (118) have attacked the problem of seeing patterns in a "noisy" background by presenting the subject with a 128 by 128 cell matrix, whose cells are assigned certain probabilities of being occupied by a dot. With such statistically defined patterns they have measured thresholds as a function of a number of standard visual variables, such as size of display and exposure time. They also studied several variables related to this kind of pattern, such as average dot probability and the graininess of the display. Thresholds decreased with increasing exposure time and were inversely related to the graininess of the display. The latter function depended on how the grain was generated, whether by changing dot size or dot separation.

Gaito (119) reports that the threshold for discriminating a straight line is lower than thresholds for curved lines and obtuse angles, and that these are lower than the threshold for detecting a box-like figure formed by three sides of a rectangle. Terrace (120) confirmed Heron's study that words are more easily recognized to the right of a fixation point than to the left. His results indicate that nonsense figures do not exhibit this directional selectivity, although the figures subtended different over-all visual angles. Using complex figures varying in shape, size, kind, and number of markings, borders, etc., Hodge (121) showed that inserting irrelevant detail into a figure increases the response latencies of classifying the figure into one of 16 patterns. These latency differences persisted throughout the stages of practice studied. In a study by Rock & Engelstein (122) it was found that the ability to recognize a figure (as a function of the time after a 20-second exposure to it) follows a different time course than does the performance of reconstructing the figure. There was little or no decrease in the probability of correct recognition over a three-week period, although there was a sharp decline in the ability to reconstruct the figure.

A somewhat different and interesting direction of analysis of the problem of shape discrimination is illustrated in two experiments by Vanderplas & Garvin (123, 124). They determined the associations, and the number of individuals giving the associations, to a variety of shapes of different complexity. The associative value of the shapes appears to be related to the correct rejection of the figure, as measured by recognition tests following paired-associates training.

*Sensory deprivation, visual pre-exposure, and figural aftereffects.*—

Held & White (125) have studied the effect of various pre-exposure conditions on the perception of apparent speed. They used four pre-exposure conditions: a hyperstable field (a single example of a random dot pattern); a dark field; a patternless, uniform field; and a "noisy" field (random dot pattern varying in time). The conditions are ordered in this way in terms of their effect on apparent speed, the first condition yielding an overestimation of speed, the others an increasing amount of underestimation. The results are interpreted as indicating that deprivation involves a randomization of sensory-neural activity rather than the absence of such activity.

Riesen & Aarons (126) have continued the experimental analysis of sensory deprivation by studying cats reared in darkness for six weeks. After this period, all animals had normal pupil reactions, lid closures, nystagmic movements, coloration of optic disk, etc. Visual placing reactions were absent in dark-reared animals, although these reactions returned with training; movement discrimination was not developed by training.

An experiment by Bergman & Gibson (127) has broadened the empirical base upon which any theory of visual aftereffects must be based. They have shown that the inspection of a figure, providing cues to slant by its textural surface, produced an aftereffect tendency to set a test figure as slanting in the opposite direction. The effect is observed with either monocular and binocular viewing, and when the test viewing is with one eye and the pre-test exposure is to the other eye. The authors feel that any adequate theory of this type of phenomenon must go beyond a discussion of contours and deal with the problem of texture.

Moed (128) found that reversing the left and right sides of the Muller-Lyer figure on alternate trials did not change the rate at which the illusion decreases in magnitude with repeated exposure. A more rapid change in the illusion did occur when subjects were given 25 long exposures rather than 50 short exposures to the figure. Cohen (129) has made extensive measurements of some of the variables that will alter the rate at which reversals occur in an ambiguous figure, the Necker cube, and has related these variables to those that will also influence figural aftereffects. There seems to be a close parallel between these two sets of variables, except in their dependence on exposure to dissimilar figures. Griffith & Spitz (130) have emphasized the importance of the texture of the field in achieving the spiral aftereffect and also point out the parallels between this phenomenon and phenomena of figural aftereffects.

*Visual search.*—A number of papers this past year have been devoted to visual sensitivity in a task involving scanning of the possible target area. Enoch (131) has measured eye movements during such search tasks and has shown that systematic biases exist in the search coverage. A marked tendency to search in the center of the search area was shown. The average interfixation distance increased and the average fixation duration decreased as the size of the display was increased. For small search areas there was a noticeable tendency to fixate outside the search area. Baker & Boyes (132)

have shown that displays can be used that capitalize on this nonuniformity of visual coverage. Ford, White & Lichtenstein (133) have used changes in the corneal potential to study eye movements during search and also report detailed data on the distributions of fixation frequency, fixation durations, and the distances between fixations. A National Academy of Science-National Research Council publication, edited by Morris & Horne (134), contains a number of papers on the applied and theoretical problems as well as data in the area of visual search.

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## HEARING<sup>1, 2, 3</sup>

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### INTRODUCTION

The trends pointed out by earlier reviewers in this field are amply confirmed. The rate of publication on the subject of hearing is increasing in a geometric progression. For example, Section 4, The Ear and Hearing, of the Analytical Subject Index of the *Journal of the Acoustical Society* accounted for 125 entries in 1958, and 230 entries in 1959. The entire field owes a special debt to this journal for its willingness to devote such a large proportion of its pages to psychoacoustic investigations, and especially, to the Associate Editors of Section 4, The Ear and Hearing: J. P. Egan and I. J. Hirsh; and Section 9, Speech and Singing: K. N. Stevens and J. F. Flanagan.

The formation of still another periodical, the *Journal of Auditory Research*, has been announced. Apparently, only psychological journals fail to reflect the growth of interest in psychoacoustics. The isolation of psychoacoustics from psychology is particularly unfortunate because the resolution of uncertainty by the psychological listener under well-defined stimulus conditions is under intensive study. A closer rapprochement of generalized behavior theory and psychoacoustics would be mutually beneficial. For example, a tool of confusion matrix analysis, developed for the examination of speech sounds (25), serves as a basic postulate of multi-choice behavior (107).

In the midst of the high level of genuine activity, it is instructive to speculate on the present state of the art in America had not the Office of Naval Research come to the aid of psychophysics in the critical years before other sources of support became available. Its present forward-looking program has been recently outlined (187). In this connection, a special debt is due to Hallowell Davis for his continuous unselfish devotion to psychophysics as an official adviser to supporting agencies, as Executive Secretary to the Committee of Hearing and Bio-Acoustics, as an active contributor to the experimental literature, and as a mentor in neurophysiology to psychoacoustics (34).

<sup>1</sup> The survey of the literature pertaining to this review was concluded in April, 1960.

<sup>2</sup> Abbreviations used in this chapter include: PTS (Persistent Threshold Shift); TTS (Temporary Threshold Shift).

<sup>3</sup> Technical Report TR 60-26 of the Air Force Command and Control Development Division in support of the ARDC Project 7682, Information Processing by the Human Operator.

Contributions outside the areas of psychoacoustics have provided tools, techniques, and results which have substantially contributed to the state of the art. Outstanding examples can be cited from biology (76), clinical audiometry (79), electronics (70), mathematics (106), physiology (11), and speech (101).

A number of major events have highlighted this reporting period and serve as recommended reading for a cross section of the field. The long awaited collation of Békésy's work has been completed (6). Békésy's new work with the skin model of the cochlea promises a further enrichment and refinement of concepts (5). Theories of hearing are becoming more and more circumscribed by the strong outpour of experimental findings (102). The theory of signal detectability, in particular, has left an imprint in experimental design, methodology, and conceptualization of the auditory process (172). Significant gains have been made in the understanding of the effects of noise upon hearing loss (114, 196), and a bright light of controversy shines in the midst of an otherwise noncontroversial, experimentally-oriented discipline (63, 162).

#### MONOGRAPHS

The highlight of this review period is Wever's translation and collation of the entire works of Békésy (6). Békésy's special genius of asking simple, direct questions and of mastering all of the necessary techniques in physics, engineering, physiology, and psychology shines forth. Experiments performed by Békésy in 1929 and 1930 are truly contemporary in 1960. Broadbent (10) summarizes experimental studies pertinent to an information processing model of the human operator. Pierce & David (129) present the facts of hearing in an understandable fashion for the general reader. Griffin (76) summarizes his own and other experiments on auditory orientation in the bat and other species. Paperback versions of the latter volumes are also available (7, 77).

#### BÉKÉSY'S SKIN MODEL OF THE COCHLEA

Békésy continues his long-term study of the cochlea by employing the skin receptors of the forearm, in conjunction with suitable transducers, as a functional model of the cochlea. A new principle of nervous action, the funneling action of nervous tissue, emerges from these studies. This concept assumes that summation and inhibition occur simultaneously in the sense organ. For example, if several closely-spaced vibrators are excited, each at the same apparent magnitude, only the vibrator in the middle is felt. The pitch is that assigned to the middle vibrator alone. There is still a contribution of the other vibrators since the apparent magnitude is reduced when the other vibrators are turned off (1). In turn, Békésy (2) establishes that the following relations hold when funneling action is relatively large: (a) the threshold is relatively low, (b) the difference limen is relatively

large, and (c) the apparent size of the sensation is relatively small. Mach's law of contrast is predicted in terms of funneling. The funneling across receptor areas that differ in sensitivity, such as in the inner and outer hair cells, has also been studied (5). As a consequence of the differences in sensitivity, the locus of sensation is continuously displaced as the intensity of the stimulus is increased. Such a displacement might provide for a pitch-loudness co-ordinate system in the ear. A wide variety of similarities between hearing and skin sensations has been uncovered by Békèsy (3). Among these are localization in space introduced by time delay, traveling waves, increase of loudness with duration, frequency sensitivity, perceived volume, inhibitory phenomena, and the concept of funneling. A related study (4) on pitch perception is considered with studies on pitch.

#### PHYSIOLOGY OF THE AUDITORY SYSTEM

*Cochlear mechanics.*—The initiation of impulses in the auditory nerve by events in cochlea is the major unsolved problem of the physiology of the cochlea (34). Recent studies of the initiation of nerve impulses in the receptors of invertebrates (11) suggest that a long series of subthreshold events intervene between transduction of a stimulus and initiation of a spike. While some of the events can be reflected in the membrane potential, others can not. These processes appear to be highly local and interact in complex ways.

A comprehensive review of the sources of electrical action in the cochlea is again provided by Davis (34). In particular, Davis suggests that the summing potential may serve "to extend the dynamic range of the ear beyond the level at which a protective limiting mechanism imposes a maximum on the a.c. cochlear microphonic."

Misrahy and collaborators (117, 118) provide a new set of techniques for the study of the oxidative processes of the cochlea. Since intense sound modifies oxygen availability to the cochlea, an alternative route to damage to the auditory system by intense sound may be available. The Misrahy group's findings prompted Spieth (152) to determine if modifications of TTS (Temporary Threshold Shift) could be observed with changes in the oxygen air supply of listeners. Spieth's findings are essentially negative. Wing (204) reviews the literature and contributes original observations to the physiology of the cochlea, placing emphasis upon the effect of oxygen change. Deatherage, Eldredge & Davis (37) review experimental work on the latency of action potentials in the cochlea. They conclude that nearly all of the well-synchronized action potential is due to neurons that arise in the first turn of the cochlea, regardless of the frequency of stimulation. It is suggested that time-of-onset information is served by the modal portion of the action potential response.

Tonndorf (182, 183, 184) continues his observations of the model cochlear partition with special reference to overload characteristics. Noteworthy



is his success in predicting the sound levels at which distortion first becomes audible in the human ear from the sound levels at which eddies become visible in the model of the cochlea (184). As a result of the phase relationships of aural harmonics of the guinea pig with differential electrodes, Tonndorf (182) concludes that "each of the newly generated harmonics forms a traveling-wave pattern of its own in exactly the same manner as do the original frequencies, i.e., fundamentals."

*Efferent system.*—The olivo-cochlear bundle (Rasmussen), which supplies efferent innervation to the Organ of Corti, has come under increasing scrutiny because of its possible inhibitory role. Ruben & Sekula (140) expand on the earliest observations of Galambos and demonstrate that, depending upon its strength, electrical stimulation of the indicated region produces a variety of effects. At low levels, stimulation abolishes responses of the cortex to clicks, leaving the eighth nerve response. At intermediate levels, stimulation abolishes activity at intermediate centers. Fex (55) further demonstrates that stimulation of the superior olivary complex not only reduces the auditory nerve response, but also modifies the cochlear microphonic and the summing potential.

*Intermediate pathways.*—Using microelectrodes at several stations in the lower system, Katsuki and collaborators (95) have simultaneously recorded cochlear nerve, dorsal cochlear nucleus, trapezoid body, and inferior colliculus. They observe that elements with a low spontaneous discharge rate are generally responsive to low-frequency tones, whereas elements with rapid spontaneous discharge respond principally to high-frequency tones. A sigmoid relationship between sound intensity and frequency of discharge is found at all stations but with a shallower slope at higher regions. A narrowing of the response area (Galambos and Davis) is observed at higher structures.

That the brachium of the inferior colliculus functionally serves more than as a pathway to the geniculate body is suggested by Goldberg, Dalen & Neff (67). They observe that a frequency discrimination habit can be relearned after ablation of the auditory neocortical areas and subsequent degeneration of the thalamic auditory nuclei. The habit cannot be relearned, however, after bilateral section of the brachium of the inferior colliculus. The cerebellar acoustic pathways have been traced by Misrahy and co-workers (119). Evidence is found for a dual sensory projection system to the cerebellum.

*Cortical system.*—Neff (123) reviews the relations between the thalamic and cortical structures of the auditory system. The function of the insular and temporal cortex ventral to the better understood primary and secondary areas is outlined. Ablation of these areas alone, as with areas A II and Ep, does not produce clear thalamic degeneration (41). Ablation of these areas, however, modifies the thalamic degeneration pattern observed with ablation of the primary auditory area. The insular and temporal cortex plays a simi-

lar interactive role in behavior. Unilateral ablation of the insular-temporal region alone does not interfere with the localization of sounds in space, but, in conjunction with unilateral ablation of the primary and secondary areas, a loss of localization results. A frequency-discrimination habit can be relearned after ablation of all known cortical auditory areas (68). In the more complex task of frequency patterned discrimination, ablation of the insular-temporal area alone results in loss of the discrimination.

The problem of adequate description of behavioral task complexity is still unsolved. To this end, Neff (123) offers an interesting hypothesis. Unlike frequency discrimination which involves the substitution of new neural elements and intensity discrimination which involves the addition or subtraction of elements, temporal discrimination (for sufficiently long signals) involves primarily the activity of the same neural elements. Neff suggests that temporal discrimination—although of the same "dimensionality" as intensity and frequency discrimination—will yield behavioral-anatomical relationships more like those of intensity or frequency discrimination. A test of this hypothesis has been promised.

Neff and collaborators (124) report that conditioned avoidance responses, established by auditory clicks, may be elicited by direct electrical stimulation of the auditory pathways. The converse procedure, conditioning to electrical stimulation and testing by auditory signals, also yields positive results. This technique should provide an extremely useful tool for exploration of the sensory systems of animals.

Desmedt & Mechelse (39) obtain evidence for corticofugal projections that suppress the cochlear nucleus response to sound. They also find evidence (40) for a fourth acoustical area which includes the ventral areas distinguished by Neff.

*Special techniques.*—The application of special purpose computers in order to separate extremely low level evoked responses from background activity is illustrated in a series of papers by the Communications Biophysics Group of the Massachusetts Institute of Technology (69, 70, 139). This group has also been active in the development of mathematical models of neural behavior (58, 139).

#### COMPARATIVE STUDY OF AUDITION

*Reptiles and amphibia.*—McGill (108) reviews the literature on the hearing of amphibians and reptiles. He finds electrophysiological evidence of activity in the auditory receptors of the lower amphibia, but only weak behavioral evidence. Strother (165), employing the cochlear microphonic, shows that the frequency range of the frog is wide (to 4 kc.p.s.) but the range of linearity is extremely low. Also employing the cochlear microphonic, Wever & Vernon (202) demonstrate a clear-cut change in auditory function within the reptile family. Reasonably good sensitivity and range are observed with crocodiles and lizards. The sensitivity of the snake and

turtle, however, is low with a sharply restricted frequency range. Despite the crudeness of the auditory structure of the turtle—a reentrant sac to permit movement of cochlear fluids—its auditory system preserves a pervasive functional pattern: the “basilar membrane lies along the path which fluid movements occur and sensory cells are stimulated in the process.” Two studies from Russian laboratories (16, 94) demonstrate that conditioned responses to tones cannot be established in the turtle except possibly at extremely low frequencies, and, then, only from high-intensity structure-bone vibrations (16).

*Birds.*—Acoustic echolocation has been observed among swiftlets of Asia (110). The frequency of echolocation is considerably lower than that of the bat or porpoise (76).

*Mammals.*—The minimum audible field of the cat (112) and guinea pig (146) is re-examined. The hearing structures of the whale, with special attention to aquatic environment, are described by Fraser & Purves (56). Acoustic isolation between the two ears is achieved by the filling of air spaces with an extremely stable foam.

Kellogg demonstrates that the porpoise can discriminate, by echo-ranging, between edible and nonedible objects (96), and between preferred and nonpreferred foods when coupled with a 2:1 size difference (97). He notes that the porpoise employs slight head movements in approaching the target. Attempts to interfere with swimming by rebroadcasting the porpoise's own signals fail (97). Similar findings are reported by Griffen & Grinnell with the bat (78).

Elliot, Stein & Harrison (52) demonstrate that the ratio of the frequency discrimination sensitivity to the total frequency range is approximately the same for cat and man. Also, absolute sensitivity is directly related to the density of innervation: the density of ganglion cells is approximately equal for the cat and man up to 2 kc.p.s. but greater for the cat at higher frequencies. Threshold sensitivity follows the same pattern.

#### THEORY OF SIGNAL DETECTABILITY

The most prominent change in psychoacoustics in the review period has been the role of theory of signal detectability in experimental design and methodology and in the conceptualization of the auditory process. While the theory was introduced to psychophysics a scant five years ago (175), it has already found application in a wide range of discrimination situations (31). With direct antecedents in radar detection theory and in the testing of statistical hypotheses, the theory is actually not specific to audition (88). For example, the index of signal discriminability ( $d'$ ) is closely related to the standard-score dispersion measure of the Thurstone scaling procedures. Detailed discussions of the theory and of its main supporting experimental studies have been presented (102), or will be shortly available (73, 172).

The approach provides a conceptual differentiation between the discrim-

inability of a sensing system and the decision processes based upon information furnished by the sensor. The failure to make this conceptual differentiation is primarily responsible for the confusion surrounding subliminal perception and other studies reporting changes of perception with various reinforcement procedures.

The signal detectability approach differs from conventional psychophysics in two important respects. First, the false alarm, or commission error, rate is not neglected. For example, for a 50 per cent level of detection, a change in the false alarm rate from  $10^{-6}$  to  $10^{-1}$  is associated with a change in discriminability that would have resulted from a 6 db change in the signal energy (73). The second departure is the denial of the concept of a sensory threshold, since an apparently continuous series of decisions is possible upon the sensory information. A rebuttal in favor of sensory thresholds has been made by Stevens (163).

A related concept is that of the ideal observer. For any given situation, the ideal observer is a theoretical performance model which can achieve optimal performance in that situation. [The particular form of the specification of the ideal observer has recently been questioned (109).] By comparison with the performance of the ideal observer, the efficiency of the listener's performance can be determined (174). For example, in the detection of pulsed signals against noise there is a striking consistency in the slope of the listener's psychometric function over a wide range of conditions (75). It is considerably steeper than that of the ideal receiver. During unfavorable signal-to-noise conditions, the efficiency of the listener is low; during favorable signal-to-noise conditions, the efficiency of the listener is high. Tanner (170) proposes that the observed difference in efficiency results from the fact that memory for various signal parameters is burdened under unfavorable conditions. With a procedure which reduces the memory requirement in the loudness discrimination task, the entire psychometric functions tend to parallel those of the ideal receiver (173). However, in the detection of a noise signal in noise, a task with no requirement for memory of signal wave-form, Green (72) obtains a psychometric function which parallels that for tone signals.

A number of other studies are related to signal memory. Swets (167) finds no difference in performance in a two-interval and an eight-interval forced-choice test. Veniar (189) finds little effect of frequency-cuing information. Related experiments report upon the effect of time uncertainty (44, 185) and frequency uncertainty (188) upon signal discriminability.

Application of the signal discriminability index to conventional forced-choice response data is illustrated by Green & Birdsall (74). [Conversion tables are found in (53).] Transforming the Miller, Heise & Lichten intelligibility data, they demonstrate that the signal detectability index is invariant over the size of message-set. If the size of the response-set is equal to, or less than, the message-set, it may be further shown that the

discriminability index is invariant over the size of the response-set, and is independent of the message-set (132).

Confusion has resulted from the fact that the signal discriminability index has been employed interchangeably in two different situations. In the first the listener must select one signal from an ensemble of possible signals. In the second the listener must, in addition, judge whether his selection was, indeed, correct or incorrect. These two situations have now been differentiated (27).

With confidence ratings, trained listeners can establish a number of decision criteria simultaneously without loss of signal discriminability (14, 46, 135). Carterette & Cole (14) demonstrate further that the basic relationships are identical with either auditory or visual stimuli. Clarke (26) demonstrates that the confidence rating adds a significant amount of information. By contrast, a second identification response provides little additional information.

Swets *et al.* (168) determine a number of properties of the trained listener. They observe that the rate of gain of signal discriminability with successive independent presentations of tones in noise is identical to that obtained by the ideal observer with perfect integration of information over successive trials. They also demonstrate a gain in discriminability with successive identical presentations, although the gain is less than that obtained with independent presentations. An estimate of the internal noise level relative to the external noise level may be made from these experimental operations. Over a range of external noise levels, the internal noise level is directly proportional to the external level. Results with speech materials (133) verify the major findings of the Swets *et al.* study, but the gain in signal discriminability with successive trials is substantially less than that predicted for the ideal observer.

Carterette (13) finds little change in intelligibility with successive presentations of each test word in noise when the listener can terminate any given trial. Similar results are observed with visual messages (15). However, it may be demonstrated (133) that either large, small, zero, or negative improvements in performance may be obtained in a listener-terminated test, depending upon the listener's criterion for termination.

Egan, Greenberg & Schulman (44, 45) present a method for examining responses to signals imbedded in noise for experimental situations in which trials are not defined. Their "method of free response" essentially obtains the entire distribution of intervals between stimuli and subsequent responses from which "responses to stimuli" and "false responses" may be determined. Because of its compatibility to the vigilance problem and to most applied situations without defined trials, the widespread application of this technique may be anticipated. Among several results of general significance is the fact that the average reaction time to signals is independent of the signal-to-noise ratio at equivalent criterial response levels.

## LOUDNESS AND LOUDNESS SCALING

Psychophysical scaling in general, and loudness scaling in particular, is one of few areas of psychoacoustics where controversy continues, despite the package designed by S. S. Stevens (159). For intensive scales, a simple relationship is found—equal sensory ratios are proportional to equal stimulus ratios. This relationship is called the "Power Law" or, in honor of its foremost advocate, "Stevens' Law."

A number of exceptions to Stevens' analysis have developed. The method of numerical estimation, which is a principal procedure in Stevens' program, comes under attack. It is argued [e.g., by Graham in the discussion of Stevens' work (157)] that, in this method, the observer determines his own numerical records rather than the experimenter. A more acceptable procedure would be for the experimenter to record the dial readings made by an observer during a matching operation.

Garner (63) voices skepticism of psychological magnitude scaling as a result of his own findings: inter-observer differences among half-loudness adjustments are extremely large; half-loudness fractionation responses by the method of constant stimulus are completely determined by stimulus context; ratios produced by listeners may bear no relation to the instructed ratios; the result of the first trial in a half-loudness constant stimulus test differs substantially from Stevens' median 10 db figure (62); the effect of context is complete within 20 trials (64); each observer's responses are internally consistent though different from other observers; and this reliability is established within 10 trials (64). In turn, Garner (63) cites the following advantages for a discriminability, or *j.n.d.*, criterion for loudness scaling: less inter-observer variability; less influence by stimulus context; and better agreement with the results of identification and category scaling experiments. Face validity is not considered to favor either approach.

An alternative theory (198) of psychophysical scaling—the physical correlate theory of sensory intensity—assumes that observers make their magnitude estimations directly in terms of sensory intensity; half-loudness or half-brightness corresponds to twice-distance of the object, or a 6 db difference per twofold apparent change. Warren, Sersen & Pores (200) observe nearly a 6-db difference with either the instruction for half-loudness or for twice-distance.

Stevens answers these objections with additional experiments and a presentation of his views (162). To the objection of numerical estimation methods, it is noted that the results obtained by numerical estimation procedures are in close agreement with those obtained by ratio production procedures, and that the relationships obtained by numerical estimation are verified when the listener is instructed to vary his force of grip upon a dynamometer in proportion to stimulus intensity (156). To those who will accept only meter readings of equation matches, Stevens adds the results of cross-modality matching experiments (161). Cross-modality matchings



may be predicted simply on the basis of power functions obtained in separate experiments with the individual modalities by the method of numerical estimation. To the reviewer, this "closing of the loop" with cross-modality matching provides a strong validation of the numerical estimation procedures.

To the question of reliability, it is argued that reliability is a relevant criterion only among alternative equally valid procedures. Questions of the validity of a procedure are not easily settled. The Stevens approach is that, if you want to know how loud a sound sounds to a person, you ask him to tell you. Is this face validity, or is this naïveté? Rosenblith (138) points out that the product of the exponent of the sensory ratio times the dynamic range in decibels tends to be relatively constant over many sense modalities. This constant may reflect important properties of the receptor sense organ, of the sensory system, or, simply of the way the listener assigns numbers.

Questions raised about context effects appear to be restricted to the use of the method of constant stimuli employed by Garner. By contrast, J. C. Stevens (155) demonstrates that numerical estimation procedures are relatively insensitive to modification in the stimulus distribution. Engen & Levy (54) also demonstrate lack of context effects upon loudness judgments employing the constant-sum method.

To the question of the relation between magnitude scales and scales derived by the category or identification techniques, it is noted that in the latter techniques the observers' ranges of responses are limited by the experimenter. Since observers often interpret their task as the assignment of different numbers to items which sound differently, the net result of such scaling is to yield a scale based almost exclusively upon a discriminability criterion. A simple relation is approached between the category and magnitude scales; the category scale is linear with the logarithm of the magnitude scale (164). Indeed, Galanter & Messick (60) have shown that the logarithmic nature of the relationship becomes more evident if the category scaling technique is extended to allow for unequal intervals and unequal discriminational dispersions. Galanter & Messick suggest further that the observer's categorical responses might reflect the ratios of his subjective scale values, instead of differences as assumed in the Thurstone model.

The logarithmic relationship between the category and magnitude functions is, according to Stevens (160), the very reason for the apparent success enjoyed by the theory of adaptation level. In essence, adaptation-level theory is correct for the wrong reasons. By assuming Fechner's law (perceived magnitude is proportional to the logarithm of stimulus intensity) and that intervals of category scales are subjectively equal, the "correct" relation between stimulus magnitude and category scales is obtained.

A special class of category scales—apparent similarity—is examined by Ekman and co-workers (49, 50). They obtain a simple relation: the apparent similarity of two stimulus objects, each expressed in units of sub-

jective magnitude, is the ratio of the less intense one to the average of the two. Weber's law is also shown to hold over a wide range when expressed in units of subjective magnitude (51).

Stevens' response to the physical correlate theory is that its validity is due to a fortuitous series of circumstances: the slopes of the brightness and loudness functions are almost equal and prediction of a 6 db per twofold change is not too different from the median result of 10 db. That nearly identical results are obtained when the listener is instructed to judge "half-loudness" and "twice-distance" is interesting, but has little relevance.

Paralleling the experimental development in loudness scaling is a theoretical development by Luce (106), which may become regarded as the most significant contribution in the entirety of scaling literature. Luce provides the possible forms of a substantive theory that relates a dependent variable in a continuous manner to an independent variable. If both variables are independent, are measured on ratio scales, and are continuous functions, the only possible relationship that will produce a set of measures, which is invariant up to a specifiable set of transformations, is Stevens' Law. If the dependent variable is an interval scale, Stevens' Law or a logarithmic function (Fechner's law) may be obtained. Thus, in one bold stroke, Luce has efficiently pared down the total universe of possible relationships to an extremely small set.

Scharf (143) finds that the loudness critical band is a critical function of the signal level. Near threshold, spreading of the noise may decrease its loudness. The full critical band is reached within 35 db of threshold. Scharf (144) also finds that the loudness of a tonal complex is independent of the number of tones of the complex when the range of frequencies is unchanged. This result poses an interesting problem for a conceptual model of loudness. A greater summation of loudness is precisely offset by a greater inhibition when a larger number of components are employed.

Thurlow & Tabory examine the effect of repeated presentations of the same tone on loudness judgments (179) and the effect of expectation of stimuli outside a defined range (169). Morikiyo (121) verifies Postman's finding of a systematic time-order error in intensity discrimination, but not in frequency discrimination. Pierrel (130) observes that gradients of generalization for auditory intensity become sharper with practice, broader with extinction, and are symmetrical in decibels whether the reinforced intensity is at the upper or lower extreme of a 40 db range.

#### PITCH

The fact that sharp frequency discrimination is possible with a peripheral system of broad sensitivity continues to command the attention of auditory theorists. Licklider's penetrating analysis (102) demonstrates the prime role of frequency selectivity in all of auditory theory.

Békésy (4) examines pitch perception on the skin model of the cochlea.

The apparent pitch drops two octaves when the vibration amplitude is increased by 40 db or the current amplitude by only 5 db. Assuming that the probability of evoking a neural response increases with intensity, an increase in apparent pitch, rather than a decrease, should result.

From this it is clear that pitch on the skin is not determined by the neural discharges synchronous with the frequency of the stimulus alone, but depends also on neural interaction of various elements.

Since phase relationships are not disturbed, something akin to demultiplication must take place at neural stations. This demultiplication would account, for example, for lower frequency of neural potentials at the cortex than at lower centers.

The determination of the upper limit of stimulus-synchronous activity to auditory signals, at the level of the auditory cortex, has been hampered by the fact that response amplitude falls off rapidly with the repetition rate. With the aid of an average response computer, which serves to cancel out background activity, Goldstein, Kiang & Brown (69) obtain stimulus-synchronous responses to clicks and noise bursts up to rates of 200/sec. in the unanesthetized cat and up to 100/sec. in the anesthetized cat.

Katsuki and collaborators (95) examine the lower auditory centers for tonotopic localization. No tonotopic organization is observed in the nerve, but, by the level of the inferior colliculus, a crude tonotopic organization is obtained. Small (148) and Kiang & Goldstein (98) find tonotopic organization at the auditory cortex, but the organization is based only upon the spectral characteristics of the signal. No representation is observed for periodicity pitch. These important results lend support to two-factor theories of pitch which require separate systems for spectral and periodicity pitch.

Theoretical papers by Pimonow (131) and Milner (116) are directed toward the problem of frequency sharpening by the auditory system. Milner proposes a series of operations in which the ratios of activities of several receptors are extracted to provide the necessary sharpening. Pimonow proposes phase sharpening as a result of the cochlear traveling wave.

Cramer & Huggins (30) examine a phenomenon in which pitch is created entirely by binaural interaction since no pitch quality is discernible when only one ear is stimulated. Eisler & Ekman (49) obtain a new pitch scale by half-pitch adjustments and, also, a similarity scale. The similarity judgments are directly predicted from the fractionation data. The pitch scale is linear with the similarity scale.

Thurlow (177) examines the time-difference pitch established by pairs of periodic pulse trains. A pitch is heard which is equal to the minimal time separation between pulse trains. The time-difference pitch is independent of the basic pulse train frequency, but is modified by a change in polarity of the pulses. Additional tests (178) demonstrate that one of the

pulse trains may be changed by 30 db with no change in pitch, although a 0.1 msec. change is clearly detectable. Thurlow & Hartman interpret these findings to rule against a simple volley theory, or, at least, "the existence of a time-analyzing system for low pitches." It should be pointed out, however, that the findings are entirely reasonable in terms of a Fourier spectral analysis of the pulse trains. Thurlow & Hartman (178) examine "the missing fundamental" for patterns of three harmonics from independent frequency generators, i.e., not locked in phase. Only a small fraction of his listeners met the frequency matching criterion imposed.

Jenkins & Harrison (92) demonstrate that the frequency generalization gradient for the pigeon is critically determined by the training procedure. Sharp frequency generalization gradients are obtained only if reinforcement in the presence of a tone is coupled with nonreinforcement in the absence of a tone. Apparently, "the establishment of a discrimination between presence and absence ensures that the tone has discriminative control over the response."

Cohen & Corso (28) observe only small changes in pitch with changes in intensity. Over most of the frequency range, such changes are insignificantly different from pitch-matching errors. An attempt to improve frequency identification by the attachment of distinctive motor responses to different frequencies yielded no better improvement than information of the correctness of the identification response (136). The information transmission with elementary auditory displays receives attention from Gershuni and associates (65, 66, 141). Effects of practice, stimulus dimensionality, and precision of discrimination upon information transmission are in agreement with the results of American investigators.

Harris *et al.* (83) study the combination of frequency and intensity changes in auditory discrimination. In an *ABX*-test listeners were presented (a) frequency differences alone, (b) intensity differences alone, or (c) intensity and frequency differences in combination. Harris *et al.* attempt to predict (c) from (a) and (b). The particular representation of the model selected was inappropriate for their forced-choice discrimination experiment. It may be shown, however, that a threshold model, such as that envisioned by Harris *et al.*, but specifically adapted to the requirements of the forced-choice procedure, is an excellent predictor of the experimental results.

#### MASKING

"What is masking?" is asked by Tanner (171) since the conventional definition—a shift in threshold as a result of experimental operations—includes clearly differentiable processes. Chistovich contributes a series of important papers to masking. Successive and simultaneous masking functions are similar for bands of noise and harmonic tone clusters (19). A technique for examining the interaction of high- and low-frequency signals

is presented (18) in terms of the regions in which either signal alone is heard and the region in which both signals are heard. The interaction of high and low frequencies is examined for both simultaneous and successive presentation (22). The frequency characteristic of masking is demonstrated by varying the level of the masked to produce a constant masking effect (21). The masking area, especially at lower masking frequencies, increases with the level of masking. [Small (149) employs the same procedure with a single pure tone at substantially lower masking levels and obtains the same general effects.] The masking of amplitude-modulated signals is shown to be a critical function of the phase relationship between the masking tone and the modulated envelope (20). The masking of clicks by clicks is examined over a wide range of inter-click intervals (23). Masking is strong within  $\pm 1.5$  msec. and decreases sharply for intervals of  $-10$  to  $+30$  msec. Backward masking, indicated by the negative durations, is associated with masking by a subsequent click. Pickett (128) examines backward masking of noise upon tones. Elevation of the tone threshold is observed for intervals to 25 msec. Pickett also summarizes earlier work in this area by Samoilova, who had presented additional observations (142) on the backward masking effect.

Bilger (8) examines the relative contribution of spectrum level and bandwidth to the remote masking of high noise frequencies upon low tones. Above a remote masking level of 30 db, a 1 db change in remote masking is associated with a 0.5 db change in spectrum level, and with a 1 db change in bandwidth. The addition of equal remote and direct masking levels results in a 6 db increase in masking (9) which is clearly greater than a simple power addition. Ehmer examines the fine structure of the masking audiogram. In the masking of tones by tones, the second peak on the masking audiogram does not necessarily coincide with the aural harmonic (47). Bands of noise produce more direct masking than tones, but roughly the equivalent extended masking (48).

The masking of echoes in speech reinforcement systems is examined by Lochner & Burger (104, 105). The effect of a delay may improve, degrade, or make no difference to speech intelligibility depending upon the relative level of the original and delayed signals. Perfect integration is achieved with 30 msec. when the delayed signal is less intense or equal to the original. A model of masking is derived from independent experiments. Its fit to the data is encouraging.

#### BINAURAL LISTENING

*Lateralization of sounds.*—Tobias & Schubert (180) differentiate between interaural temporal disparities to the carrier signal (ongoing disparity) and temporal disparities to the envelope (transient disparity). Previous failure to make this distinction has caused confusion. Transient disparities which favored localization in one direction were pitted against

ongoing disparities which favored localization in the opposite direction. The effect of transient disparity is relatively greater for brief signals than for long signals. For a given signal duration, the ratio of the required ongoing disparity to the required transient disparity is relatively constant. The effective onset duration—the product of the ratio by the signal duration—is constant at about 3 msec. Tobias & Zerlin (181) introduce both onset and transient disparity between the ears. For durations of 300 msec. and longer, the mean interaural temporal disparity required for consistent lateralization was less than 6  $\mu$ sec.

Schubert & Elpern (145) estimate the velocity of the cochlear wave by masking different lengths of the basilar membrane of the separate ears and delaying the earlier emerging click to achieve centering. The velocity of the cochlear wave is estimated at 55 m./sec. near the stapes and about 30 m./sec. at a point 20 mm. distant. Deatherage & Hirsh (38) also demonstrate that the effect of high-frequency noise in one ear upon a click may be offset by a time delay of the click in the opposite ear and that the size of the offset is related to the noise intensity.

The time-intensity trading relation for brief acoustic stimuli is found to be a function of the absolute sound levels employed (33, 38). Results are consistent with the assumption of a physiological intensity-to-time conversion: if the two ears are offset by a given temporal difference, a smaller intensity disparity will be required at low signal levels than at high.

The relative roles of interaural time and intensity disparities have been studied for both impulsive sounds and pure tones as a function of frequency. Mills (115) determines the minimal intensity differential required for lateralization of pure tones, without temporal disparity, and compares the findings with the localization of acoustic sources in space. The intensity differential required for lateralization equals that required for localization for frequencies between 1500 and 6000 c.p.s. At lower frequencies, the two functions differ greatly. Harris (81) and David, Guttman & Bergeijk (32) also indicate that a change in slope of the trading relation is obtained as a function of frequency. The critical region for the change of slope is around 1500 c.p.s.

A technique for determining the time-intensity relationships in binaural localization is furnished by Moushegian & Jeffress (122) in terms of a "noise pointer" which can be moved in apparent space by adjustment of a delay line. When time and intensity disparities are opposed, time has less effect upon lateralization than when time and intensity favor the same side.

An exchange of comments (17, 32) results in the distinction between studies in binaural fusion (involving primarily ongoing disparities) and binaural lateralization (involving primarily transient disparities). In the place of a coincidence comparator as a model appropriate to transient information, models for binaural fusion employ moment-to-moment proc-



essing, such as running cross correlators (17). Cross-correlation models are of special relevance to the discrimination of differences in the cross correlation of broad-band noise (137). The sensitivity for a change in the cross correlation is much greater for correlations near 1.0 than for correlations near zero. This behavior tends to parallel the sampling variability of the correlational coefficient. The optimal period between successive brief correlated noise samples for the identification of interaural correlations is found (134) to be about 2 to 3 msec.—approximately the same size as the effective onset duration of Tobias & Schubert.

*Localization of sounds in space.*—Hanson (80) has demonstrated that in a reverberant room where conflicting cues are possible extremely large precedence effects may be observed. Thus, if a tone is first presented to loudspeaker *A* and then gradually and smoothly changed to loudspeaker *B*, localization will persist at *A* for many seconds after the shift has been made to *B*.

Jonkees & Verr (93) repeat an experiment by Klensch on the relative contribution of head and neck movements to binaural hearing. Each ear was connected to a funnel through a tube. Poor directional sensitivity in the median plane is observed as long as the head and funnel are immobile. Head movements are important in terms of their synchronization with the funnel movement. Burger (12) examines the role of head movements and directional characteristics of the outer ear in front-back discrimination. Front-back discrimination is sharply reduced when the head is clamped, and covering the outer ear tends to reduce discrimination primarily at high frequencies.

Leakey (100) studies the effects of interchannel intensity and time differences on the localization of sounds produced by two-channel sound systems. Only somewhat smaller interchannel differences are required for low-pass signals than high-pass signals. A geometric model of the signal configuration suggests the importance of small head movements, and appears to predict the experimental findings well.

*Cross-masking and interaural phase effects.*—Large contralateral effects upon the intensity discrimination of weak sounds are observed by Chocholle (24). Ingham (91) observes cross-masking effects at low levels with continuous noninterrupted tones. Frequency-selective cross-masking effects are observed at 1000 c.p.s. but not at 200 c.p.s. Moray (120) finds that a contralateral speech signal interferes little with the reproduction of a desired speech signal when its intensity is equal to, or less than, the desired signal at the opposite ear. Hirsh & Burgeat (90) examine interaural phase effects with the remote masking of low tones by a high-frequency noise band. Unlike normal masking, phase reversal of the tone is substantially more effective than phase reversal of the noise. The results suggest phase distortion is produced in the remote masking process. Small interaural phase effects are observed at the absolute threshold (42). As with masked thresholds, the

antiphasic condition is superior. The data are consistent with the masked threshold experiments if it is assumed that the listener's internal noise contribution is homophasic in terms of its effective representation at the two ears. Durlach (43) presents a functional model of the binaural listening system based upon interaural time differences.

#### NOISE-INDUCED HEARING LOSS

How much noise can man be exposed to without permanently affecting his hearing? A model survey under field conditions is provided by the ANEHIN (auditory and nonauditory effects of high intensity noise) project (35). Of special note is the "method of single descent" for group audiometry.

Experimental studies of noise-induced hearing loss are concerned with temporary threshold shifts (TTS) and persistent threshold shifts (PTS). Gravendeel & Plomp (71) question the relation between TTS and PTS because of differences in the frequency at which maximal shifts are observed to small-arms firing: 4.6 kc.p.s. for TTS and 6 kc.p.s. for PTS. A shift in the threshold shift vs. frequency plot between PTS and TTS is observed (114) with PTSs first appearing at lower frequencies than TTSs.

Can noise conditions, which alone produce no TTS, interact with other conditions which produce a TTS? Trittipoe (186) demonstrates small, but reliable, effects by noise levels which alone produce no TTS. Ward (190) has repeated Trittipoe's experiments—with somewhat younger observers—and obtains negative results.

The Subcommittee on Noise has placed the study of TTS upon a quantitative framework in which specific parameters may be evaluated. It demonstrates (193) that, for moderate noise levels, TTS is logarithmically related to the duration of noise exposure and to the recovery duration, and is directly related to the on-fraction of the noise. The growth and recovery of TTS following octave-band noise exposure parallel the course of TTS following broad-band noise exposure (194). The course of recovery of TTS is uniquely determined by the TTS measured at a post-exposure duration of 2 min. (i.e., after the initial recovery process has run its course) for a wide range of conditions (194). The effects of interrupted noises can be described in terms of their equivalent exposure duration (195). The effect of alternating between two noise levels is considered (192). It is demonstrated that, at high levels of TTS, the logarithmic recovery curve is maintained for about 10 hr., but, thereafter, recovers at a rate which is linear with longer recovery durations (191). A research program which complements that of the Subcommittee on Noise is provided by Spieth & Trittipoe (153, 154).

Miller and associates have bridged the gap between TTS and PTS by producing both types of shifts in cats. The following points are noted (114): cats show more TTS and PTS than men for corresponding noise expo-

tures; the basic features of the cat's TTS audiogram are the same as man's; PTS is an increasing function of the logarithm of the exposure duration; the PTS findings are reflected in the histopathology of the cochlea; and the temporal distribution of the noise is extremely critical. Miller & Watson (113) demonstrate that the longer the interval between successive noise exposures, the lower is the PTS. An interval of only one hour between successive exposures substantially reduces the average PTS. This result may have significance in terms of the removal of workers from noise environments for rest periods.

The characteristic hearing-loss-with-age curves demonstrate larger threshold shifts with age for males than females (86, 87). Ward, Glorig & Sklar (197) inquire if males are, perhaps, more susceptible to the effects of noise. They observe no significant difference in TTS between young males and females following noise exposure. Assuming a monotonic relation between TTS and PTS, one must look elsewhere, e.g., greater noise exposure or faster aging, for the greater threshold shifts in the male population. Most workers (86, 87, 197) lean toward greater noise exposure, especially that of small arms, as the prime factor for the male-female difference. For example, there is a substantial correlation between threshold shift and number of rounds of small arms firing for the exposed ear, but the correlation is weak for the opposite ear (86).

Kylin (99) defines an exposure criterion to octave bands of noise in terms of the octave band level, presented for two hours, which yields a TTS of 10 db, 15 min. following noise exposure. Such a criterion agrees closely with a damage risk criterion suggested by Hardy in 1952. The permissible (as defined) band levels fall at the rate of 4 db/octave above the 75 to 150 c.p.s. band to the mid-range of frequencies. Thompson & Gales (176) demonstrate that the TTS associated with a given exposure of a critical band of noise is equivalent to that produced by the corresponding exposure of a tone in the center of the band. Harris (82) examines the TTS following exposure to high-frequency pulse trains. A new unit, the NOX, representing the cumulative TTS over a 10 min. interval was found to be more reliable than the TTS at 2 min. post recovery time. For rapidly interrupted sounds, equal sound energy yielded equal NOX scores. Harris reviews deafness risk criteria for pure tones and noise and concludes that the previously presented pure tone criteria may have been somewhat conservative to noise criteria. Despite the rapid advances in the field, it should be noted that no test has yet been validated which will permit the industrial screening of susceptible persons to noise-induced hearing loss (166).

#### AUDIOMETRY

Hallpike & Hood (79) continue to elucidate the basic auditory processes by their carefully controlled studies of clinical material. They define the loudness recruitment angle in terms of the slope of the recruitment func-

tion. Within close limits, this angle varies linearly with the amount of hearing loss. Support is found in their clinical material for a dual excitation theory, as enunciated by Davis.

Hinchcliffe (86, 87) examines the threshold of hearing in rural populations. Noteworthy is the testing of 100 per cent samples and the verification of the British Standard for the normal threshold of hearing in the survey setting. The decrement in acuity with age for the female group is considered as pure presbycusis. The decrement of acuity with age for other senses tends to parallel that for hearing (85).

Palva (126) demonstrates the diagnostic value of whispered vs. voice audiometry. Whispered speech, which emphasizes the contribution of the high speech frequencies, is 6 db more effective for normal and conductive-deaf subjects but not more effective for perceptive-deaf subjects. The confusion surrounding hearing loss vanishes with the proposed distinction (36) between "hearing level" (audiometric reference level), "hearing loss" (clinical symptom of reduced hearing), and "threshold shift" (individual's own threshold).

#### RESPONSE TO LOW FREQUENCIES

The response of the ear to low frequencies (below 200 c.p.s.) has largely been ignored. A few encouraging exceptions have reversed the trend.

The absolute threshold for low-frequency tones (5 to 200 c.p.s.) is determined by Corso (29). Above 25 c.p.s., the threshold falls at 21 db/octave, which is in agreement with previous work, but the threshold for 5 c.p.s. is still uncertain. Small (149) also examines the threshold for low-frequency tones and examines the masking produced by tones down to 20 c.p.s. Pickett (127) demonstrates significant decrements in speech intelligibility due to noise frequencies between 20 and 300 c.p.s., although removal of the same range of speech frequencies produces no change in intelligibility.

Green, McKelvey & Licklider (75) examine the masking of pure tones by a noise of uniform spectrum. Unlike earlier results, the tone-to-spectrum noise level uniformly decreases to 250 c.p.s. Durlach (43) finds that the size of the homophasic-antiphase masking level difference uniformly increases as the tonal frequency is reduced to 100 c.p.s. At low frequencies, the antiphase-monaural difference is greater than the difference between the listener's monaural masked threshold and that of the ideal observer of the theory of signal detectability.

#### SPEECH

Extensive resources are being devoted to the solution of a problem: the recognition of speech by computers, which is, in large part, a psychological problem. Indeed, the development of a speech typewriter is employed by

Miller (111) as the vehicle for organizing gaps in the field of speech and language.

The present state of the art of speech recognition has been summarized (201). Noteworthy are the approaches of Frick (57) in making a large number of crude decisions to achieve a recognition program; of Stevens (158) in achieving recognition by synthesis and matching; and of Smith (150) in performing a pattern analysis without phonemic recognition.

As speech processing systems become more sophisticated, tests other than intelligibility will be required. For example, certain operations may seriously affect the quality of the system without modifying intelligibility (147). Some of the variables important for speech stress (59, 103) and speech quality (125) have only recently been examined.

Two important papers from the Haskins Laboratory throw light upon the speech recognition process. The rules for synthesis of speech are codified (101), and the precision of discrimination among synthetic speech sounds is shown to be determined entirely by the listener's ability to identify the sounds (84).

#### UNCLASSIFIED

Among several important papers that do not fit within the topical organization of the review, Gardner & Licklider (61) report on their experience with a loud noise as a dental analgesic. Hirsh (89) finds that one sound must lead another by at least 15 to 20 msec. for discrimination of temporal order, the required duration being relatively independent of the spectral characteristics of the signal. White (203) finds that simple multiplicative transformations on audio frequency do not seriously disturb the recognition of simple melodies. The semantic differential approach is applied to the characterization of sonar sounds by Solomon (151). And Warren & Gregory (199) describe an auditory analogue of the visual reversible figure.

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## THE CHEMICAL SENSES<sup>1,2</sup>

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During the past five years there has been an accelerated increase in the number of papers related to the chemical senses. Electron microscopy has been applied to the minute structures of both the gustatory and olfactory receptors, and some traditional structures, such as taste hairs, have now been classified as artifacts. Much more attention has been given to the odorous stimulus and its presentation to the subject. Complex natural stimuli have been analyzed with gas chromatography, and a wealth of information concerning the concentration and type of chemicals found in flavors has been accumulated. The quantitative response to odorous stimuli has been investigated by measuring the magnitude of newly discovered potentials, such as olfactory receptor potentials, or the neural activity recorded from the primary olfactory nerves. The responses of central neural processes concerned with gustation and olfaction, including centrifugal influences upon the olfactory system, have also been studied.

Much of the pertinent information to date concerning taste and smell has been summarized in two chapters of the recently edited *Handbook of Physiology* (1, 2). The roles of olfaction and gustation in flavor research have been reviewed in the book *Flavor Research and Food Acceptance* (3). The present author has made no attempt to be all-inclusive in this review. Only about 25 per cent of the total papers relating to the chemical senses and published during the past five years have been selected to be included in the bibliography.

### GUSTATION

*Anatomy of the taste structures.*—With the application of electron microscopy and histochemistry to taste structures, our knowledge of their anatomy has advanced farther in the past five years than in the previous four decades, and minute details, many of which may be directly related to taste function, have been revealed (4, 5, 6). De Lorenzo (5) identifies both taste and sustentacular cells in the taste bud, whereas Engstrom & Rytznar (6) could find no clear distinction between cell types within the taste bud. It is very probable that one type of cell may gradually change into another. If, as this implies, the taste cells are being replaced continuously so that all the cells in the taste bud are really transitional taste cells, then colchicine injected into an animal should interfere with taste function, since this chemical has been used extensively to prevent mitotic division of cells. In

<sup>1</sup>The survey of the literature pertaining to this review was concluded in April, 1960.

<sup>2</sup>Abbreviations used in this chapter include PTC (phenylthiourea).

fact, the magnitude of electrophysiological response to taste stimuli in the rat was shown by Beidler *et al.* (7) to decrease 50 per cent within four hours after injection of colchicine and to disappear completely within 8 to 10 hours. Isotopically labeled thymidine, a component of chromosomal nucleic acid, may be used to label cells undergoing mitotic division. When tritiated thymidine is injected intraperitoneally into a rat, autoradiographs show that the cells of the outer layer of the taste bud are continually undergoing mitotic division (7).

Since the classical taste hair has not been observed with electron microscopy, it is thought to be an artifact (4, 5, 6). However, the apical process of many of the cells of the taste bud bears numerous microvilli, each of which is about  $2\mu$  long and  $.2\mu$  in width, and extends into the taste pore.

The taste nerves of mammals are about  $4\mu$  in diameter and enter a nerve plexus, located directly beneath the taste bud, where the nerves lose their myelin, branch profusely, and enter the bud as axons varying in diameter from  $.04$ – $.3\mu$  (1, 2). These bare nerves terminate in clublike endings, which are separated from the surrounding taste cells by a distance of about  $.03\mu$ . One synaptic ending may be in contiguity with several taste cells (5, 6). Cholinesterase, an enzyme closely associated with neural activity, has been isolated histochemically in the basal portions of the taste bud, in the nerve fibers supplying the taste bud, and in high concentration in the nerve plexus (8, 9). These findings indicate that the synaptic endings are not stimulated directly by the taste-provoking substances, but that the taste cells mediate the response. Further evidence of the importance of an acetylcholine transmitting mechanism at the synapse is given by Skouby *et al.* (10) who found that acetylcholine applied to the tongue decreases the taste threshold to electrical stimulation by 20 to 60 per cent.

Although earlier workers found rudimentary taste buds in foetal material, Gairns (11) found no taste buds in the soft and hard palates and uvula of the human adult. A few taste buds were found by Burkl (12) in the middle third of the esophagus of man. All present evidence supports the view that the taste nerve is necessary to maintain the integrity of the taste bud in mammals but not in lower forms such as the newt (13 to 17).

*Taste fiber specificity.*—Cohen *et al.* (18) analyzed the response of single taste fibers of the cat to several taste stimuli and identified four fiber types—water, salt, quinine, and acid. One would, of course, assume a sugar fiber in those mammals that, unlike the cat, have a good response to sugar. Pfaffmann (19) questions the meaning of fiber-type classifications and states that no simple classification of taste receptors by types is obvious. He studied the unitary response of taste receptors in several different species of animals and concluded that each single fiber preparation is characterized by a different pattern of sensitivity to the four common taste stimuli. Pfaffmann states:

Chemical specificity appears to be relative, so that any one element might be characterized by the stimulus or stimuli for which it has the lowest threshold. Other stimuli at higher concentrations might be included in the groups of chemicals to which it is sensitive.

It would appear that with such studies it is necessary to use a large variety of chemical stimuli at different concentrations. Beidler (21), Pfaffmann (19), and Fishman (20) showed that classification of salt fibers depends upon the salt chosen as a stimulus and upon the species studied.

Zotterman (22, 23) raises the question of whether water has a specific taste. He points out that those animals that possess only a few salt fibers responding to low concentrations of salt also possess a large number of water fibers responding to distilled water. In the latter case, low concentrations of salt may be detected by abolition of the spontaneous activity due to distilled water, and, thus, the range over which an animal might discriminate salts is extended. It is interesting to note that Wolbarsht (24) suggests that the chemosensory hairs of the blowfly might also respond to distilled water.

It appears that the neural organization of the chemoreceptors of insects is simpler than that found in mammals. Hodgson & Roeder (25), for example, recorded from the two neurons which innervate the chemoreceptor area in the tip of the labellar hair of various diptera. One nerve produces small potentials, and its receptor responds only to sugars; whereas the other receptor neuron produces large potentials, and its receptor responds to salts, acids, and alcohols. The former receptor is associated with acceptance and ingestion of a particular substance, whereas the latter is associated with avoidance or rejection. The third receptor, normally found with the other two receptors, responds only to mechanical stimulation (26). The remnants of a two-fiber system are also found in the frog, as described by Zotterman (27). The large fibers are much more sensitive to water than to the monovalent salts; the opposite is true of the small fibers. As one studies the response of mammals, such distinctions become vague, although in some mammals the small fibers respond well to bitter substances.

*Central nervous system.*—Cohen *et al.* (28) and Landgren (29) recorded evoked potentials in the cerebral cortex of the cat in response to stimulation of the tongue. Several cells were found to respond to both taste and either stretch or touch; to stretch and cooling; or to stretch, warming, and cooling (29). Thus, it is concluded that there is a great convergence of tactile, thermal, and gustatory impulses in the central nervous system. Benjamin & Emmers (30), however, found an area of the cortex, responding exclusively to taste stimuli, in a position rostral to that normally activated by tactile stimulation of the tongue. Although this work was performed with the squirrel monkey, it was suggested that the true taste area in the cat might be hidden in the pre-Sylvian fissure and that

the previous work on the cat was probably done in the tongue tactile area and not in the area devoted exclusively to taste. A thalamic center devoted exclusively to taste was localized by Emmers *et al.* (31) in a subnucleus of the most ventral and posterior part of the *Nematus ventralis*. Only very slight overlap of tactile response was observed in this area of the rabbit. Pfaffmann (32) recorded the electrical activity in the chorda tympani nerve from the tongue, and in the medullary and thalamic sensory relays for taste, after chemical stimulation of the tongue. Both at the peripheral and subcortical levels physiological activity is found to be a rising function of stimulus intensity. Behavioral responses to substances like sucrose were found to be related directly to the strength of sensory discharge.

*Species differences.*—In the past, the relative response to a series of different taste stimuli was considered to be identical in different species. This was derived from the belief that the intensity of taste response depends upon a single physicochemical characteristic of the stimulus. For example, response to various cations was thought to be similar in various species of animals if ionic mobility is a major factor in taste stimulation (33). With the increased use of electrophysiological techniques, a great interest has been shown in the study of taste responses of various species of animals. Thus, in sheep and calves no response to water has been detected (34, 35); in the guinea pig, hamster, and dog the response is small; and in the rabbit, pigeon, and chicken it is large (36, 37, 38). The response to sucrose varies from very small in the cat, to moderate in the dog and rat, to rather large in the hamster, guinea pig, and rabbit. Carnivores such as dogs, cats, and raccoons, as well as the opossum, respond much better to potassium chloride than to sodium chloride at equimolar concentrations, whereas the reverse is true for rodents and the bat (36, 39). Pfaffmann (40) also studied the taste responses of the rat, cat, and rabbit and found the relative effectiveness of the three electrolytes, HCl, KCl, and NaCl, in suprathreshold concentration to be different in the three species. The relation between the magnitude of neural activity and a behavioral response to the same stimulus is not yet clear.

The chicken may have as many as 24 taste buds (41). Kare *et al.* (42) measured the chick's intake of flavored solution, as compared to the intake of water, and concluded that the chick has a sense of taste which is more than rudimentary, but that the taste preferences are not identical with those of man. A very small neural response to sodium chloride, sucrose, and saccharine and a good response to glycerine and quinine were recorded from the taste nerve of the chicken (37). The pigeon responds to high concentrations of sodium chloride, acetic acid, and saccharine but not to sucrose or quinine (37).

The relation of the taste responses of lower forms of mammals to those of man is uncertain. Gordon *et al.* (43) recorded the neural activity from the taste nerve of the rhesus monkey and observed a good response to

water, sodium chloride, acid, quinine, and sugar. The fibers in the monkey responding to sucrose almost always respond to saccharine as well, which is not the case for many other mammals. The neural response of the ring-tail monkey to taste stimuli is much more representative of the carnivores than that of the rodents (44). The chorda tympani nerve in man passes through the middle ear and is available for electrophysiological study during middle ear surgery. Zotterman & Diamant (45) recorded from this nerve in man and obtained good responses to salt, sugar, saccharine, quinine, and acetic acid, but no response to water, in contrast to the good response obtained from monkeys. Such electrophysiological studies in the human hold great promise if correlated directly with psychophysical reports from the patient.

Taste substances used in mammalian experiments are usually found unsuitable for lower forms, since such substances are not natural to their environment. Barber (46) recorded the electrical response of the taste nerves of *Limulus* and found that the receptors are relatively unresponsive to common salt, sweet, sour, and bitter solutions, but respond strongly to aqueous extract of marine bivalves. This paper emphasizes the danger, when studying the sense of taste in other forms of animals, of using stimuli to which man naturally responds. In almost all of the studies to date, it would have been better if a greater variety of stimuli were used over wider ranges of concentrations. Some attention should always be given to the type of stimulus normally found in the environment of the animal. Arthropod chemical sensitivity was related by Hodgson (47, 48) to the natural history of the animals. The butterfly has been shown to respond to the four common taste stimuli (49).

*Psychophysical studies.*—The Weber fraction in man was obtained by Schutz & Pilgrim (50). The value for salt is .15, for sweet .17, for sour .125, and for bitter .30. The fraction is constant for all five bitter intensities but differs near the absolute threshold for salt, sour, and sweet, and near the terminal threshold for salt. Individuals differ widely in their differential sensitivity to the basic tastes within the qualities and in the order of their sensitivity to different qualities.

A logarithmic relation between the magnitude of subjective sweetness and concentration was established by Schutz & Pilgrim (51). Relative sweetness of taste compounds with respect to that of sucrose changes as the subjective intensity or concentration is increased. Information theory was used by Beebe-Center *et al.* (52) to analyze absolute judgments of sucrose and saline solutions. The channel capacity for both substances is almost two bits, which Miller (53) shows to be slightly less distinctive than that for auditory stimuli. For saline solutions, the measure of information transmitted appears to be a maximum when about four alternative concentrations are used. Beebe-Center *et al.* (54) measured the intensive equivalents for sucrose and sodium chloride solutions over several logarithmic



units of concentrations and found the effectiveness of saline solution to be almost twice that of sucrose solution calculated on a molecular basis.

Interaction of different taste qualities is found with both subthreshold and suprathreshold values of the basic taste stimuli (55, 56). Calcium cyclamate enhances the sweetness of sucrose at medium concentrations but not at low or high (57). Monosodium glutamate may raise the absolute threshold for sweet and sour, but does not increase their subjective intensities (58).

The effect of stimulus area upon taste intensity was studied by Hara (59), who found that the threshold concentrations of saccharose, saccharine, sodium chloride, acetic acid, magnesium sulfate, and quinine hydrochloride are an exponential function of the negative value of the area stimulated. The reaction time to suprathreshold concentrations decreases linearly with the area of the tongue stimulated and logarithmically with the concentration of the stimulus.

Tilgner & Barylko-Pikielna (60) measured taste thresholds of 392 subjects for the four taste qualities. The average threshold covered the range of .1 to .4 per cent for sucrose, .1 to .18 per cent for sodium chloride, .008 to .017 per cent for tartaric acid, and .0036 to .0038 per cent for caffeine. Women showed higher sensitivity than men for sweet and salty materials, and lower sensitivity for sour, but no sex difference was noted for bitter substances.

*Effect of age and genetic constitution.*—An eyelid conditioned response to taste stimuli was studied by Osepian (61) in children up to 18 months old. Fewer stimuli are needed to initiate the response as the child develops up to the third month, and this response is formed at an equal rate between the third and ninth months. Differentiation of flavored solutions appears at the third month. Osepian (62) also studied a conditioned general motor reflex in puppies, which is first established at the age of 14 to 19 days, in response to sugar and table salt. Differentiation of taste stimuli is seen at the age of 35 to 40 days. Flasarova (63) could find no reliable orientation of the newborn in response to the four basic taste stimuli during the first day of life. Thereafter, the newborn acts uniformly to raised concentrations of taste solutions, although the thresholds are much higher than in adults. A change of taste sensitivity in elderly humans has been studied by a number of workers with conflicting results (60, 64 to 67).

Brandtzaeg (68), using 176 subjects, confirms the hypothesis that taste dimorphism and sensitivity to phenylthiourea (PTC) depend upon a main pair of genes in which the recessive one in homozygous condition causes the lack of tasting. Population distribution of PTC threshold is truly bimodal and the antimodal value, which discriminates tasters from nontasters, differs according to populations studied (69 to 72).

Lugg (73) and Das (72) report that thresholds are lower for homozygous than for heterozygous tasters. Kalmus (74) improved the classi-

fication of taster genotypes by correlating the PTC threshold with that for quinine in order to correct for general sensitivity to bitter substances regardless of genetic considerations.

Lugg (75) reports a PTC threshold of  $6.5 \times 10^{-8}$  molar, involving an estimate of  $8 \times 10^9$  molecules, which is by far the lowest taste threshold reported in the literature.

*Radiation effects.*—Water and food consumption of rats are depressed during exposure to a relatively low dose of low-intensity gamma radiation. Garcia (76) showed a conditioned aversion of rats to saccharine resulting from exposure to gamma radiation affecting an undetermined site. The response of the rat to radiation and taste stimuli following ophthallectomy indicated that this site is not that of the phosphene of the eye but may be referred to changes in gastrointestinal function during the exposure period (77, 78). Kalmus & Farnsworth (79) reported that radiation of the posterior area of the tongue and the oral pharynx decreases taste sensitivity to sodium chloride, sucrose, PTC, quinine, and picric acid during the period of radiation, and that taste sensitivity recovers gradually over several months. Taste nerves may be affected; although, for unknown reasons, response to acid stimulation is very small.

*Interdependence of food intake and taste.*—Young (80) discusses psychological factors regulating the feeding process and reports that both rats and men prefer higher sugar concentrations to lower if only a brief contact with those substances is made. However, if the volume of intake is used to determine relative acceptability, one finds an optimal concentration of sucrose. Carpenter (81) obtained relative intake curves for sucrose and sodium chloride in the rat, using both a two-bottle method in which concentrations were presented in a random order and an eight-bottle method in which all concentrations were presented simultaneously. With the former method, peak intake occurs at isotonic concentrations, whereas with the latter method, the highest concentration of sucrose is ingested in greatest amount in contrast with the greater importance of lower concentrations for sodium chloride.

Young (82) found that thirsty rats reject hypertonic salt solutions and tend to prefer water to hypotonic salt solutions. Yensen (83, 84) rendered two subjects deficient in salt and studied their sensitivity to sodium chloride, sucrose, sulfuric acid, and quinine sulfate. A decrease in threshold for sodium chloride was the only change noted during the salt deficiency period, and during the recovery period this returned to normal. He later studied the effect of water deprivation on taste sensitivity and found that the sodium chloride threshold changed, whereas that to sweet, sour, and bitter stimuli did not (85, 86). Salt taste threshold falls sharply with greatly increased water consumption (87). After the intake of food an immediate decrease in taste sensitivity occurs which is related to the caloric intake of fasting subjects (84). The period of increasing appetite is accompanied by

decreasing taste sensitivity. Zaiko (88) studied the response of single taste papillae stimulated with a capillary tube containing various taste solutions and found a decline in sensitivity after eating. After 1½ hours, the number of active papillae had increased somewhat; four hours after eating, it approximated the initial number. Finding that filling the stomach with sweet tea inactivates the taste buds, Zaiko concluded that the digestive system sends signals concerning its own state to the gustatory apparatus. Grossman (89) suggests that stimulation of the receptors in the region of the head by the smelling, tasting, chewing, and swallowing of food plays an important role in bringing about suppression of further eating but that this factor may be relatively ineffective when it is not associated with entry of food into the stomach.

Changes in the internal environment of an animal may also affect its appetite. Adrenalectomized rats show a specific appetite only for sodium chloride, and neither lithium chloride nor other salts studied can be substituted (90). A reduction of blood-sugar level by injection of insulin produces no differential change between sodium chloride and sucrose response, as recorded electrophysiologically by Pfaffmann & Hagstrom (91). Therefore, the enhanced sugar preference shown in behavioral studies following the injection of insulin is not associated with a change in taste receptor sensitivity but probably is of significance in the central neural processes (92).

Specific ablation experiments also indicate the importance of the central nervous system in the regulation of food intake. Soulairac & Soulairac (93) produced cortical lesions, in areas 6 and 10, in the rat which did not interfere with the gustatory area. Such lesions increase the caloric intake, if the rat is given a choice of food and glucose, but decrease both the intake of, and behavioral sensitivity to, glucose. They suggested that it is possible to decrease the behavioral sensitivity to glucose even though the receptor sensitivity has not changed. Benjamin (94) studied the effect of fluid deprivation on taste deficits following cortical lesions and concluded that the cortical taste area is necessary for normal taste discrimination only under conditions of low fluid deprivation. Thus, the determination of whether ablation of the chorda tympani projection area of the cortex produces deficits in taste discriminations depends upon the type of discrimination test employed. Anderson & Jewell (95) electrically stimulated the thalamic relay for taste and the posteromedial ventral nucleus in the goat and observed a response identical to that observed when the animal gets a distasteful solution in the mouth. After discrete bilateral lesions destroying its nucleus, complete loss of taste is observed. Brobeck (96), in a review article, claims that all that is definitely known about neural regulation of food by the hypothalamus can be accounted for on the grounds that the hypothalamus is an integrative center for impulses brought into

the nervous system from sensory cells in other parts of the body, rather than a site of primary receptors.

The behavioral responses of insects appear to be of much simpler origin than those of mammals. Dethier (97, 98, 99), in a series of very interesting papers, cites numerous observations on the factors important in the ingestion of carbohydrates by the blowfly. No relation is found between the amount of sugar ingested by insects and nutritive value. The proboscis is extended as soon as the fly encounters food and the chemosensory hairs in the tarsi are stimulated. Feeding then begins, and the oral receptors adapt slowly. Threshold is regulated by the neural activity passing from the sensory receptors in the foregut to the brain where it inhibits the effect of the sensory input of the oral receptors. During hunger there is an absence of inhibitory impulses carried by the nerve from the foregut. The blood-sugar level, stored glycogen depletion, crop and midgut content, and humoral agents in the blood do not affect threshold regulation. Thus, summation of feeding is not brought about by fulfillment of metabolic need since a fly fed a nonmetabolizing sugar will have its threshold response elevated and feeding terminated.

The glucose threshold of the blowfly is shown to decrease with increasing duration of flight, an effect which could not be related by Hudson (100) to a change in blood sugar, crop sugar, nor glycogen depletion. Injection of glucose into exhausted flies enables them to resume flight almost immediately, but the injections do not bring about changes in the sucrose threshold.

*Mechanisms of taste stimulation.*—The single taste nerve fiber synapses with a number of single taste cells in the taste bud. Each single fiber of the chorda tympani nerve is known to respond to a number of stimuli representing several of the four common taste qualities, but does a single taste cell respond only to one taste quality or to several? Kimura & Beidler (101) thrust microelectrodes into a taste bud of the rat tongue and measured changes in resting potentials when a number of salt solutions or stimuli representing the four basic taste qualities were applied to the tongue. As in the single-fiber experiments, no simple classification of receptors in terms of the potential changes evoked by the four basic stimuli is obvious.

Using electrophysiological methods of recording, Beidler (102) studied the rat neural taste response to 19 different organic and inorganic acids. Concentrations for equal response varied from 2.2 to 150 mM, whereas the hydrogen ion concentration varied from 2.6 to 6.0. Greater response of organic acids over hydrochloric acid at equal pH can be accounted for quantitatively by the facilitative action of the undissociated acid which is adsorbed to the sites of the receptor.

Dethier reviews the various current theories of taste and concludes that it is highly probable that the mechanism of taste receptor stimulation is more nearly biophysical than biochemical in action (103). Enzyme theories

have, however, been suggested by various people, particularly since histochemical studies have shown an increase in certain enzyme concentrations in the taste area as compared to other areas of the tongue. For example, alkaline phosphatase shows strong enzyme activity in the taste-bud cells, the taste canals, and the microvilli, but not in the gutter or surface epithelium (8). The specificity of the reaction involving this enzyme is not great; Ellis (8) demonstrated that a synthetic substrate for this enzyme could be substituted for naturally occurring substrates. The role, if any, of such enzymes in the mechanism of taste stimulation is not known. When metabolic substrates such as malonate are added to the perfusion fluid of the frog tongue, the electrical neural response to glucose, sodium chloride, and tap and distilled water is either reduced or eliminated. But both succinate and fumarate prevent malonate from having this effect according to Koshitoin & Katalin (104). On the other hand, Lawrence & Ferguson (105) added enzyme inhibitors to sweet solutions in order to determine whether the taste substances interact directly with a receptor enzyme. Neither sodium azide, potassium flouride, sodium iodoacetate, nor sodium cyanide (all known metabolic inhibitors) obliterated the sweet taste irreversibly. It is concluded that the taste receptor surface is not that of an enzyme. Four sweet substances were then adsorbed to charcoal and to human hair, and the quantitative data were found to fit Freundlich's adsorption equation. A large number of sweet substances was rated according to relative sweetness by a taste panel and then examined for physicochemical factors that might be involved in taste stimulation, but none was found to be correlated directly with the intensity of sweetness.

If the primary taste mechanism is enzymatic in nature, then taste sensitivity should be very dependent upon temperature. Dethier & Arab (106) found that the action of chemical stimuli at the tip of the tarsal and labellar hairs of the blowfly is independent of temperature over a very wide range, in contradiction to the observation by Hodgson & Roeder (25). If the taste substances are adsorbed to receptor sites, it should be possible to obtain inhibitors that compete for the same site. Warren & Pfaffmann (107) studied the inhibitory action of gymenic acid upon the ability of man to taste sucrose and saccharine. Threshold sensitivity for both sucrose and saccharine change to the same degree with gymnemic acid, and equisweetness matches are unaffected. The inhibitory effect levels off at higher concentration of the gymnemate. The purified gymnemic acid was found to be a glycoside, and it was theorized that the sugar fragment of the molecule enables it to compete with sweet substances for sites on the taste cell which are essential for stimulation.

Kusano & Sato (108) showed that a single fungiform papilla of the frog can be stimulated by touch, pressure, and the four fundamental kinds of taste solution, as well as by distilled water. They used a micropipette containing the taste stimulus to stimulate a single fungiform papilla and

recorded electrophysiologically from the taste nerve. Cocaine and alopoin may abolish the response to ordinary monovalent salts but not the response to water. This suggests that the receptor area responding to water is different from that responding to monovalent salts (109). The effects of cations and anions on the activity of the taste receptors of the frog and toad were also studied (110). Morita & Yasmashita (111) investigated the inhibitory effects of one stimulus upon another by measuring the depolarization of the receptor membrane of the chemosensory hairs of flies upon stimulation with sucrose and sodium chloride. A hyperpolarization was recorded when calcium chloride, quinine, and acetic acid, all of which have an inhibitory effect on the initiation of chemosensory activity, were applied to the receptor.

Beidler (112) applied his previously derived theory of taste, which accounted for the quantitative taste data obtained electrophysiologically, to certain psychophysical properties of human taste sensation. It was assumed that each taste cell contains a number of sites to which various chemical stimuli can be adsorbed. A simple taste equation relating the magnitude of taste sensation to the concentration of the stimulus accounts for the relation between the just noticeable difference thresholds and the stimulus concentration. Taste threshold was found to be dependent upon the equilibrium constant of the reaction and the maximum response obtainable at high concentrations. The latter is very dependent upon the physiological and psychological conditions of the experiment. Ichioka & Hara (113) studied the quantitative relations between the reaction time of the human gustatory sensation and the stimulus. The relation is based upon Beidler's earlier theory that the electrophysiological response magnitude is almost a logarithmic function of the concentration over the medium concentration range.

#### OLFACTION

*Anatomy of the olfactory epithelium.*—The densely packed olfactory receptors range from 62,000/mm.<sup>2</sup> in the pig to 127,000/mm.<sup>2</sup> in the rabbit, with a total of 100 million in the latter (114, 115). The nuclei of the receptors are situated at different depths in the olfactory mucosa with olfactory rods 20 to 90 $\mu$  in length extending to the surface. Such differences in receptor morphology may be related to a differential sensitivity to odors and, thus, may be a possible basis for odor quality discrimination (116, 117).

The olfactory rod terminates in a swelling from which 2 to 16 olfactory cilia, depending upon the species, extend into the mucus. The lengths of these cilia, formerly called olfactory hairs, are still being debated but may range from 2 to over 40 $\mu$  (114, 115, 118). Each olfactory cilium contains nine pairs of oriented filaments spaced around the margin of two others centrally placed; this is typical of the internal structure of many other types of motile cilia found elsewhere in the body (119).

The central process of the olfactory receptor cell is the olfactory un-



myelinated axon of about  $.2\mu$  in diameter. These axons conduct at the slow velocity of .2 meter per second and are classified as C fibers by Gasser (114). Each olfactory receptor connects by its axon to the olfactory bulb where it synapses to another fiber that goes directly to the cerebral cortex. Hence, LeGros Clark (116) states:

There is only one synapse intervening between the impingement of a stimulus on the olfactory receptor and the arrival of the nerve impulse at the cerebral cortex. A directness of connection that far transcends that of any other sensory system.

Upon lesion of the olfactory bulb, approximately half of the receptors undergo rapid degeneration and disappear (116).

Olfactory acuity is often thought to be related to the amount of yellow pigment found in the olfactory epithelium. However, Gerebtzoff & Philippot (120) suggest that pigment in the olfactory area may be needed only in those animals that normally have an odorous gas flowing over the olfactory regions since they found no evidence for pigment in the olfactory mucosa or Jacobson's organ of fish and urodeles. Moulton (121) failed to show any difference between trained pigmented and albino rats in their olfactory acuity to the odors of normal ethyl, butyl, and hexyl alcohols. Chemical analysis of the pigment fraction by Philippot & Gerebtzoff (122, 123) and Jackson (124) indicates that it is associated with a phospholipid, probably a lecithin. Jackson fractionated the pigment and concluded that the color is due to auto-oxidation products of phospholipid origin, and, thus, the pigment is a mixed, unstable oxidation product. The degree of pigmentation varies both within and between species.

*Electrical activity of olfactory structures.*—Various potentials recorded from different areas of the olfactory system have been studied by a number of authors and have been related to odorous stimuli. Ottoson (125, 126) recorded slow negative potentials in the frog, toad, and rabbit with an electrode placed upon the olfactory mucosa as various odors were flowed over the area. He believed these potentials to be generated by the olfactory receptors and electrotonically conducted to the bulb. The function of such conduction is not clear, since Beidler & Tucker (127) and Kimura (128) have shown that the olfactory axons conduct with action potentials in a manner similar to other sensory nerves.

The characteristics of the slow potentials of the mucosa appear to differ with the observer. The potentials obtained by Loed (129) in response to repetitive stimulation show an exponential decline and are quite different from those obtained with continuous stimulation by Ottoson (125). Slow potentials recorded by Takagi & Shibuya (130) show "on" responses, "on-off" responses, and "off" responses, which are not reported by other investigators. The reason for these differences is not clear but should be investigated.

Gross, recording from the surface of the olfactory bulb, also indicates

slow potentials in response to odor stimulation as well as olfactory waves (125, 131). The waves were first thought by Adrian to be of receptor origin, but further consideration shows them to be generated by the dendritic potentials in the glomerular region (132). Gedewanishvilli (133) permanently implanted electrodes in the olfactory bulb of rabbits and cats and found a rather stable wave frequency of 45 to 55 c.p.s. in the awake animal, which drops to 10 to 15 c.p.s. under medium anesthesia and disappears under deep anesthesia. Mozell & Pfaffmann (134) and Mozell (135) used carefully controlled olfactory stimuli in the form of amyl acetate, heptane, ethyl ether, and benzene, and recorded the quantitative response of the spike discharge in the olfactory bulb. Discharge strength and duration increased approximately as the negatively accelerated function of concentration over a range of 1 to  $1\frac{1}{2}$  logarithmic units. A gross bulbar anterior-posterior spatial differentiation of cell types was found. Electrode depth in the spike layers does not affect the spatial-temporal and concentration relationships.

Electrical waves of 25 to 39 c.p.s. from the human olfactory bulb were recorded with depth electrodes by Sem-Jacobsen *et al.* (136). The responses recorded simultaneously in both bulbs to various odors were neither synchronous nor of the same amplitude.

Adrian (137, 138) recorded from mitral cell units and found that one unit may respond to a given concentration of acetone and of eucalyptus while another responds to a lower concentration of eucalyptus and a higher concentration of acetone. Thus, differential odor sensitivity exists at the mitral cell level, which might account for odor quality discrimination in the rabbit. The cells may be grouped as those particularly sensitive to aromatic compounds, esters or fatty acids, paraffin hydrocarbons, terpenes, or sulfur-containing compounds. Adrian (138) states that differential receptor sensitivity is not the only, nor necessarily the most, important factor in odor identification. Other factors such as time course and distribution of excitation over the different areas of the olfactory organ may be very important. These factors may merely reflect the way in which different odor molecules distribute over the olfactory surface.

Adrian (138) suggests that the brain may receive olfactory information conveyed in the form of sinusoidal waves by the route of the grey matter as well as by the independent discharges in the large mitral fibers in the olfactory tract. The latter may provide fine discrimination, whereas the former route may be concerned (principally) with arousing olfactory attention and control of motor reactions, such as inspiration, needed for effective use of the olfactory organ. He also suggests that the process may be aided by signals from nonolfactory receptors in the snout, nasal cavity, respiratory apparatus, etc.

The resting and odor-induced electrical activities of the olfactory bulb are depressed when the basal rhinocephalic area, or anterior commissure, is

excited (139). These centrifugal influences may be tonic. Lavin *et al.* (140) implanted multipolar electrodes in the olfactory bulb of the cat and found that the electrical activity of the bulb is increased when the unanesthetized animal is stimulated by visual, acoustic, somatic or gustatory stimulation. Electrical stimulation of the mesencephalic reticular formation produces an arousal discharge of the olfactory bulb. The intrinsic activity of the olfactory bulb may also be altered by a change in its osmotic pressure (141). Since this sensitivity does not depend upon an intact olfactory epithelium nor come from other areas of the brain, Sundsten & Sawyer (141) concluded that there are osmosensitive elements in the olfactory bulb of the dog.

*Odor perception in mammals.*—The olfactory threshold of the rat was measured by Eayrs & Moulton (142) in three different experimental situations. No rat showed any signs of learning on a Y-maze, although some rats succeeded in performing olfactory discrimination in the rectangular choice apparatus. Learning was retarded by nonolfactory factors. The circular choice apparatus was shown to be excellent for the study of olfactory acuity. Eayrs & Moulton concluded that formation of learned habits based on olfaction depends upon the close contiguity of stimulus and reward and that the sense of smell possesses weak orienting properties in comparison with those exerted by other modalities. It was determined that odor detectability by the rat, in the circular choice apparatus, increased logarithmically with the number of carbon atoms in the molecule (143). The patterns of detectability, however, are dependent upon the response criterion chosen. If an 85 per cent success level is used, there is a clear trend towards oscillation in the response as the series is ascended; this trend is not present if 50 per cent success level is used. Great variability exists between members of the same species of animals, and, thus, the necessity for a large number of subjects in olfactory determination is very important. Although many workers have indicated that dog odor thresholds are many times lower than those for man, Becker *et al.* (144) could not train dogs to respond to concentrations of oil of cloves below those detectable by man. Dog thresholds are lower when the handler knows the position of the odor than when he does not, which indicates the necessity of the careful control of nonolfactory cues (144, 145).

Neuhaus (146) measured the differential threshold of dog and man to odors of butyric acid and found that the values decline with increasing concentration, attain a minimum, and then begin to increase again. Extreme values for the dog are 0.100 and 0.86, whereas for man they are 0.35 and 1.60. He showed (147) that dogs can distinguish two different odor qualities, even though both are at the same odor intensity. No discrimination between butyric and propionic acids is possible at minimal detectable threshold concentrations, but discrimination is possible at slightly higher concentrations.

*Comparative studies.*—Occluding the olfactory pits of sunfish with latex, Gunning (148) observed behavior in a situation where homing would occur, and concluded that olfaction is important in the mediation of homing. Teichmann (149) trained eels by means of food reward to respond to a number of synthetic olfactory materials. The eel possesses great ability to differentiate between various chemicals and shows great sensitivity. For example, the eel responds to  $\beta$ -phenylethyl alcohol in a dilution of  $1:2857 \times 10^{-18}$ . This corresponds to approximately 1766 molecules/cc. of water. Although the eel may need up to 10 sec. to make a choice, its olfactory sensitivity is approximately the same as that of the dog as determined by Neuhaus (150). The salmon also has great chemical sensitivity and responds to minute dilutions of L-Serine (151), which is normally present in the scent of certain fish (152). The arousal of feeding activity by electrical stimulation of the brain indicates that the olfactory, rather than the gustatory, system plays the predominant role in feeding behavior of fish (153).

Schneider (154, 155) recorded electrophysiologically from the antennae of moths and found great sensitivity in males but not in females to an odor obtained from the scent gland of female moths. Takeda (156) obtained a conditioned reflex to the odor of hydroxycitronellal using as the unconditioned reflex the proboscis extension reaction to 1.5 molar sucrose applied to the tarsi of the honeybee.

An interesting paper by Hughes (157) shows that the length of time during which a fly is in flight gives a measure of its activation by an odor and may be used to study olfactory acuity.

*Factors influencing olfactory response.*—The sniffing technique of odor presentation was compared by Jones (158, 159, 160) to the controlled blast technique in measuring the recognition thresholds for several odors. Sniffing thresholds were lower and appeared to be better for rapid and simple determinations, whereas the blast technique appeared better for comparing thresholds for different substances under rigidly controlled conditions. Jones was unable to relate the absolute thresholds of 20 different odorous substances to any one physicochemical property.

In most quantitative work on the olfactory sense so-called pure chemical substances are used since accurate descriptions of the odor can be given and their physicochemical properties are well defined. The olfactory receptors respond to very minute concentrations, and, therefore, odor contaminants are always important. Cheesman & Townsend (161) found a 400 per cent variation in olfactory threshold when samples of so-called pure isopropanol taken from two different bottles (both reagent grade and obtained from the same manufacturer with identical stated impurities) were used as the stimuli. Mullins (162) obtained odors from the National Bureau of Standards and redistilled them to avoid possible impurities.

Olfactory data obtained from different subjects vary considerably. For

example, Myznikov (163) presented an identical odor of thymol to two different dogs of the same strain and found that the threshold concentrations differed by a factor of 500 million. Moulton *et al.* (143), using two dogs to determine the relative detectability of normal aliphatic acids, found that in one dog the detectability tended to increase logarithmically as the series ascended, but that in the other dog this relation was discontinuous.

The dependence of olfactory response on flow rate has been studied quantitatively in the rabbit and man (162, 164, 165). This dependence must be a consequence of the mechanism of olfactory stimulation since, when the odor is passed directly over the olfactory epithelium, the response is still flow-rate dependent. The flow rate over the olfactory epithelium is dependent not only upon the flow rate through the nares but also upon the extent of nasal vasodilation. Tucker & Beidler (166) showed that vasoconstriction by sympathetic stimulation results in increased olfactory response to a given concentration of odor. Thus, any olfactory measure is greatly dependent upon the physiological state of the nose, which may account for the large variations in olfactory thresholds between individuals. Vishnepolskii (167) demonstrated that both the olfactory and trigeminal nerves respond to variations of the nasal cavity pressure, regulate respiration, and, therefore, may affect the olfactory response.

Olfactory thresholds may be affected by hormones but, since they may also reflect changes in vasodilation of nasal structures, the primary site concerned with the threshold changes is not known. Schneider *et al.* (168, 169) showed that the threshold value for citral almost doubles during menstruation and that estrogens improve and androgens depress olfactory acuity in hypogonadal women.

Olfactory thresholds for coffee and citral increase after the age of 60 according to Megighian (170). The hedonic value of an odor may also vary with age. Stein *et al.* (171) found that almost all three- and four-year-old children rated the odor of synthetic feces and synthetic sweat as pleasant, whereas all the children between the ages of five and 12 rated them unpleasant.

**Adaptation.**—The magnitude of neural activity recorded electrophysiologically from the primary olfactory nerves is constant during successive inspirations at low odor stimulation, whereas it declines to a steady level with successive inspirations at medium concentration, and complete adaptation is observed at high odor concentrations (172). Adrian (173) emphasized that the olfactory bulb is of greater influence than the receptor in determining sensory adaptation.

Adaptation to a given odor concentration was measured by Moncrieff (174, 175) and compared to the rate of adaptation following exposure to another odorous substance. He concluded that a high degree of cross-adaptation is found only when the two odorous substances have a smell so simi-

lar that they may be easily confused. The classification of smells into a small number of groups by the method of adaptation appears impossible.

Stuiver (176) determined, under conditions of normal breathing, the time required for cessation of smell sensation with prolonged odorous stimulation and found that the adaptation time was approximately linear to odor concentration at high stimulus intensities. The recovery of smell as measured by threshold concentration after previous adaptation is rapid initially and is followed by a slower recovery. The rise of threshold, measured after a given time of adaptation, is approximately linear to the stimulus intensity.

*Psychophysics.*—Engen & Pfaffmann (177), using the method of absolute judgment of odor, determined that a human subject can identify by label about 16 odor qualities. Informational analysis of the results yielded four bits. They also determined that an unpracticed subject can identify three levels of intensity of an odorant, whereas a well-practiced subject can identify about four levels (178). This corresponds roughly to a transmission of  $1\frac{1}{2}$  bits of information about odor intensity differences as compared to the four bits of information about quality differences.

Using the method of magnitude estimation, Jones (179, 180) determined the subjective intensities of a number of odors at various levels of concentration. The subjective intensity was related to the stimulus magnitude by a power function with an exponent of about .5 which did not vary with the type of odor used.

The critical fusion frequency of several odors was measured by Blondal (181) at various concentrations. For each odor, the relation between the fusion frequency and concentration involves two linear parts, each part obeying Fechner's logarithmic law of response.

Stuiver (176) determined the threshold of several different mercaptans and calculated that  $9 \times 10^6$  molecules were necessary to reach threshold. Since  $2 \times 10^7$  sensory receptors are estimated as available, only every other cell receives an odorous molecule, provided the molecules are evenly distributed over the olfactory epithelium. By measuring the probability of smelling for a given concentration of mercaptan, it is shown that a receptor needs nine molecules or less to respond. From the curves for smelling vs. concentration, it was also determined that a minimum of 40 sense cells are stimulated at threshold concentration.

Pfaffmann *et al.* (182) have designed an olfactometer for the study of olfactory discrimination, which permits a Skinnerian conditioning procedure. The rat faces an air stream in a glass wind tunnel that contains a bar-pressing apparatus and dipper mechanism to provide occasional water reinforcement for pressing the bar in the absence of odor. A well-trained rat stops bar pressing immediately after a suprathreshold odor is introduced.



*Trigeminal stimulation.*—The trigeminal nerve innervates the nasal region including the septum and turbinates. The trigeminal sensory nerves can be stimulated by a number of different odors which are not necessarily obnoxious (183). Although many such odors stimulate the olfactory receptors at concentrations lower than those required for the trigeminal, some odors, such as phenylethyl alcohol, may actually stimulate the trigeminal nerve of the rabbit at lower concentration than the olfactory receptors. The usual criterion in human psychophysical experiments, the presence or absence of pain, is not sufficient for determining whether the trigeminal component is responding to an odor. The role of the trigeminal nerve in the production of odor sensations is unknown. Beidler & Tucker (172) recorded simultaneously from strands of the olfactory and trigeminal nerves of the rabbit in response to a given odor. The olfactory receptors respond only during inspiration, whereas the trigeminal nerves respond more slowly and activity is maintained during inspiration and expiration. The olfactory threshold of the rabbit to amyl acetate was measured electrophysiologically as .5 mM, but the trigeminal threshold to the same odor was 3.0 mM.

*Olfactory theories.*—Davies & Taylor (184, 185) proposed an interesting theory to account for the magnitude of olfactory thresholds based upon the molecular shape, size, and partition properties of the odorous molecules. It was assumed that a critical area of 64 square angstroms of the cell surface must be dislocated by the presence of the adsorbed odorous molecule in order to initiate a receptor response. About 44,000 such areas exist on each receptor. With the strongest odorant, beta ionone, only one adsorbed molecule is necessary within the critical area, whereas for very weak odors up to 24 molecules are necessary. The ability to bind odorous molecules to the receptor is predicted from lipid-water interfacial adsorption data. The number of odorous molecules available to a given site when a predetermined concentration of odor is flowed over the olfactory receptor is given by the familiar Poisson expression. The theory accounts for the olfactory threshold of a number of substances, but does not take into account odor quality or the relation between intensity of sensation and odor concentration.

Wright *et al.* (186) postulated that the pigment molecules of the receptor cells can exist in an electronically excited state if supplied with the necessary energy from cellular metabolic processes. When an odorous molecule is adsorbed to the pigment, it is assumed that de-excitation of the pigment molecule occurs and a redistribution of receptor surface charge effects a breakdown of the membrane potential, which in turn initiates the nerve impulse. Twenty-four different pigment types, contained in as many end organs, are found necessary by Wright *et al.* to account for various odor qualities. The theory does not predict any quantitative relation between properties of the odorous molecule and olfactory sensation.

In order to obtain information concerning the internal attractive forces between the receptor membrane molecules, Mullins (162) measured the olfactory thresholds for homologous series of paraffins, alcohols, and chloroparaffins, all having different solubility parameters. From these experiments it is postulated that there exist in the olfactory epithelium at least two, and probably more, types of receptors, each of which possesses a different solubility parameter or internal attractive forces that hold the molecules of the membranes together. In addition to the property of solubility parameter, molecular shape of the molecules is also important in determining olfactory threshold. It is theorized that the receptor membrane contains a number of pores of different size randomly distributed over the receptor surface. An odorous molecule of the same size as the pore will excite this portion of the receptor surface but will narcotize all large pores. The concentration necessary to excite some receptor types is more than sufficient to narcotize others; consequently, the total response declines and adaptation appears. Small molecules such as xenon enter all the pores and quickly narcotize them so that no odor sensation is ever present. Very large molecules do not find a sufficient number of suitable pores to completely excite the membrane, thus they have no odor.

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## PERSONNEL SELECTION<sup>1</sup>

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### INTRODUCTION

Like job analysis and attitude surveys, personnel selection is one of the traditional areas of industrial psychology. Historically, the work of the late Walter Dill Scott and his colleagues in the Army during World War I gave psychological testing, which was soon to become synonymous with personnel selection, its initial impetus. Perhaps because it has become traditional, personnel selection lacks the glamour of some of the more recent activities of the psychologist in industry and has suffered lately in the quantity and imagination of effort devoted to it. Possibly, age and respectability have made personnel selection a mundane activity as compared with the more dramatic areas of management development, group dynamics, human relations training, human engineering, and the like. In spite of its apparent drabness, personnel selection remains an extremely challenging area of psychology and one of fundamental and growing importance in a business world of increasing complexity, and any decrease in the interest of psychologists to work in this area must be viewed with alarm.

In our changing industrial environment, two factors appear to these writers to make selection more important than ever. First is the increased job stability at both hourly and managerial levels. There appears to have been a trend since the end of World War II for management to separate fewer personnel than had previously been the case. One reason for this is, of course, the growth of labor organizations. To an ever-increasing degree labor contracts are restricting management's freedom with respect to personnel actions. Initial selection is one of the few areas in which management has not yet surrendered its prerogatives.

Second, mechanization and automation are in the process of reducing the number of simple, repetitive tasks that can be quickly and inexpensively taught to new employees. The more demanding jobs of the automated factory, although fewer in number, necessarily require more careful and accurate selection. Not only has it become more costly to train the replacement for an unsuccessful operator, but the increased complexity of his task has made the cost of errors he might make much, much greater than those of his preautomation counterparts. The growing complexity of jobs and their increased training costs are equally true at the managerial level, where many firms invest as much as two years of intensive training on a management recruit before he even begins to become productive. Thus, a single

<sup>1</sup>The survey of the literature pertaining to this review was concluded in April, 1960.

selection error can easily cost a company tens of thousands of dollars.

But the selection psychologist's obligations are not solely to the employing organization. As humanitarian scientists we have obligations as well to society and to the dignity and welfare of the individuals involved. A number of psychologists, such as Argyris (1), Likert (37), and McGregor (41), have addressed themselves to these points in the broad framework of the business organization or with particular reference to performance appraisal. Outside of the counseling situation, in which the individual is the client, there is little reference to the selection psychologist's role in this respect. While warned by Gellerman (21) that we must give greater protection to the testee in the revelation of personality test results or face a possible revolt against them, we find nowhere in the literature any allusion to the ways in which valid test selection procedures operate to benefit the applicants (even the rejected ones) as well as the company. There appears to be a real need for studies that follow up rejected job applicants and compare their fate with that of matched samples of equally qualified but selected applicants.

Popularly, personnel selection has been (to many psychologists as well as to business managers and employees) only initial selection. In a much broader sense a personnel selection takes place, not only when an individual is first hired by a company, but on each occasion that he is promoted, transferred, or reassigned. Except for the selection of first-level supervisors, this broader definition seems to be rather generally ignored in making personnel actions. Thus, in surveying the literature, we find a complete void of studies on the relationship of success or failure in one position as it relates to another. This absence of research is true even of the military, where the rotation of career officers every two or three years is a firmly established practice. We suggest that the internal movement of personnel, particularly in the management ranks within an organization, offers a fruitful and, as yet, largely untapped field for selection research.

These are but a few of the problems that remain to be tackled after this year's results are in. It may be that these brief comments will rekindle needed dedication in this field. On the other side of the ledger, the comment must be made that some of the recent work has been of high quality and importance. Perhaps we ought to be a little more awed by the fact that there is as much research in personnel selection as does take place, and ought to be less critical of our failures. By its very nature, research with personnel selection devices is almost always necessarily actuarial. This fact frequently brings the requirements of good research design into direct conflict with the practicality of a situation. Relatively few industrial organizations have sufficient numbers of personnel in identical jobs to make feasible the conduct of good validation studies, even on a concurrent basis. The industrial psychologist, however competent and conscientious, is almost always faced with the need to compromise what he knows to be good re-

search procedure in the interest of providing his sponsor with advice and recommendations which, if not founded on the results of ideally designed research, are at least based on empirical evidence that can be made available within the limitations imposed upon it by reality. It does not seem unreasonable to conclude that, in spite of the fact that in altogether too many instances the design or results of a study, or both, are poor, a healthy degree of research activity in connection with the development and validation of tests and other personnel selection devices and procedures still persists. With this in mind, we can look at some of the major findings of the past several years.

#### TESTS AND PERSONNEL SELECTION

*The Mental Measurements Yearbook.*—The year 1959 witnessed the publication of *The Fifth Mental Measurements Yearbook* (12) edited, as usual, by Buros. This Gargantuan volume lists 957 tests, 698 of which are given original reviews. Most of the remainder are tests that had been reviewed in an earlier edition of this series. There are, in addition, according to the editor's preface, "6468 references on the construction, use and limitations of specific tests." The impression that one obtains from sampling this 1292 page work is that, by and large, psychological tests are primarily educational and counseling tools. Even among the 114 entries in the section entitled "Vocations," relatively few of the entries were developed, standardized, and validated on either employed personnel or job applicants. The same is true to an even greater extent of the 96 nonprojective tests of character and personality. With the exception of the Activity Vector Analysis, the validity of which is seriously questioned by both of its reviewers, no nonprojective personality test appears to have been constructed specifically and adequately for industrial personnel selection use. To a large degree the same is true of intelligence and aptitude tests, and the industrial psychologist seeking suitable devices to validate in the selection situation finds relatively few designed for this purpose.

Even the newer forced-choice personality tests, as characterized by the Edwards Personal Preference Schedule and Gordon's Personal Profile and Personal Inventory, appear to fall far short of what is needed. Borislow (9) demonstrated the fakeability of the Edwards while Corah *et al.* (14) found that the preference indices of selected items broke down upon being paired. Reviewing the Edwards Personal Preference Schedule, Barron (12, p. 117) says, "Judging from the literature on the PPS at this date, the verdict of caution would be that the test is not yet ready for use in counseling or personnel selection." Nevis & Parker (56) demonstrated that neither the college male norms nor adult norms published in the most recent manual of the Edwards (17) were appropriate for selection use.

Gordon's tests, too, are rather severely criticized by their reviewers. Concerning the Personal Profile, Fricke (12, p. 128) says,



since the Profile became available commercially in 1953, it is perhaps significant that the reviewer was unable to locate one study in the literature bearing on the test's validity; not only have individuals other than the author not reported on its validity, but the author himself has not done so. If the claims that the author makes for the test are even partially substantiated by other investigators, it would definitely be a test worth using.

Concerning the Personal Inventory the same reviewer says,

Despite the lack of validity data, the manual for the inventory, as did the manual for the profile, presents many suggestions in the section on uses of the test. The author claims, for example, that the inventory "has certain attributes which give it unique potentialities for use in personnel activities, such as the selection of individuals for specific types of work, the placement of individuals in specific jobs, and the counseling or transferring of employees not performing well in their present job (12, p. 125).

Such claims, without any basis in empirically established validity, are not unique to this instrument. They are, unfortunately, the rule rather than the exception.

Thus, *The Fifth Mental Measurements Yearbook* is, in many ways, discouraging to the psychologist engaged in personnel selection in industry; nevertheless it is a sound, informative, and critical publication that should not escape the attention of anyone engaged in this field.

*New test directions.*—Several publications during the past two years are of interest more for future validation studies than for validity reported. A seminar on "Objective Approaches on Personality Assessments," edited for publication by Bass & Berg (5), reports several possibilities in this regard. Berg, for example, makes a good case for the use of new types of items and tests based upon response patterns or sets. This would lead to instruments which lack the face validity emphasized so much in currently popular tests, but could result in new and exciting possibilities in test design. Miller, Hunt, and Cattell, in the same symposium, report studies where such possibilities have been attempted, Miller going so far as to use "brass instrument" and electronic tests as a means of obtaining more precise measurements. In this same volume Holtzman reports the use of a new set of ink blots designed systematically to enhance the concepts of form and color developed by Rorschach. By working toward a more sound instrument from a methodological point of view, it may be possible to obtain greater predictive validity from the potentially rich projective techniques. [Nevis, Wallen & Stickel (57), working under the direction of the senior reviewer, have developed a semiobjective approach to the Thematic Apperception Test which is in some respects parallel to Holtzman's work.]

Fredricksen, Saunders & Wand (20) report on the in-basket test. This situational device goes in the opposite direction from that suggested by Berg in that it uses material highly related to the actual performance tasks of management personnel. Their study shows the difficulties in obtaining

reliable measures, and they suggest a number of ways in which this type of material can be refined to be more useful. Unfortunately, psychologists have been using the in-basket technique more as a training device than as a selection measure, and the literature shows no validation studies with it up to this point.

Another paper of more general interest in test prediction is that of Becker (6). Evaluating the conclusion of Cattell and his co-workers that their research has demonstrated almost one-to-one correspondence between questionnaire personality tests and behavior ratings, Becker comes to the conclusion that such may not be the case and that another approach should be considered. He believes that Cattell's behavior-rating factors have now been sufficiently replicated on enough populations to be seriously considered as criteria for questionnaire development. From this approach comes the terms of a behavior-factor criterion. The differences in the two kinds of either a self-perception factor (*à la* typical questionnaire approaches) or in terms of a behavior-factor criterion. The differences in the two kinds of scores could provide interesting possibilities about possible areas of conflict or defensiveness and relate to greater selectivity in using the devices in prediction studies.

#### PERSONNEL SELECTION IN INDUSTRY

*Methods other than tests.*—The past few years have seen the appearance of a number of new books on interviewing, most of which have been reported in previous *Annual Reviews*. All of these publications are of a "how-to-do-it" nature, and none reports any validation studies of this most frequently used selection device. There have been, however, a number of articles over recent years showing favorable results with biographical information data of one kind or another.

Hinrichs (30) reported that the average of several selection interview ratings on nine factors for a group of 70 scientific and technical personnel had no relationship whatsoever to subsequent performance. He then demonstrated on a sample of 239 technical personnel that it was possible to predict subsequent performance on the basis of a specially designed weighted blank. His expectancy table is based, evidently, on his item analysis sample, but he reports that "similar" results were obtained on an independent sample. Scollay (64), conducting further research on a previously validated personal history form, was able, on the basis of 68 weighted items, to predict the success of the assistant district sales managers in promotional capacities. Incidentally, his study is another indication that unit weights result in higher validities than do differential weights. Working with a variety of office jobs, Kirchner & Dunnette (35) were able to identify long- and short-term employees on a similar basis.

In a poorly designed experiment with an inadequate number of cases, Harrell (29) attempted the validation of five "knock out" factors advocated

by a mail-order testing firm. The reported results are vitiated by the fact that the food company studied had been rejecting applicants on the basis of these items, but that because of labor market conditions or the possession by certain applicants of what local sales supervisors considered redeeming characteristics, some men with "knock out" factors had been selected.

The weight of the evidence seems to indicate that biographical information items of one kind or another remain one of our best predictors. In this respect the authors would like to suggest further research in this area using a more dynamic approach to the weighting of items. A genotypic scoring, such as that advocated by Worthington (82) and used successfully by Nevis (55) and others, might lead to even greater prediction than the traditional approach to weightings.

Two studies using employment reference forms are worthy of mention. Goheen & Mosel (27) found that an objective score from the Employment Recommendation Questionnaire used by the Civil Service Commission of the United States did have some relationship to ratings made by field investigators on the basis of interviews of persons familiar with the candidate. The findings showed that different data were revealed by these two approaches and that the questionnaire did not identify extreme disqualifying features such as alcoholism or other factors which might be used to disqualify candidates. In a related study Mosel & Goheen (49) attempted to validate the Employment Recommendation Questionnaire against various kinds of other references and found little or no relationships. Over-all, these studies and previous ones by these authors confirm the belief long held by practitioners that questionnaire attempts at reference checking are very little more than useless.

*Procedural contributions.*—While the reviewers have proceeded on the assumption that highly technical questions such as new statistical concepts were beyond the limits of this review, several papers are worth mentioning from the standpoint of modified or new procedures which could lead to better selection research. Ferguson (19) presents a logical and geometric analysis of the relationship of interest and ability in predicting production and survival of insurance agents. He comes to the conclusion that aptitude is a combination of ability and interest factors. He furthermore states that, in predicting success, the greater the time interval between initial selection and criterion measurement, the more important will be interest factors and the less important will be ability considerations. The latter conclusion is presented mainly as an hypothesis for further research. It must be considered in terms of Ferguson's definitions of ability and interest. However, this distinction may prove quite valuable in selection situations, such as with sales personnel, where the long-term tenure is of great concern. In the interest sphere, Stone (70) developed a measure of vocational interest which he related to intraoccupational proficiency. His criterion of vocational success was pure shorthand skill, measured by means of a job sample.

One hundred and ninety of the 400 items of the Strong Vocational Interest Blank differentiate the top 25 per cent and bottom 25 per cent of about 1000 students. These results held on both a validation and cross-validation sample, indicating that weights based on extreme groups within an occupation might predict as well as, or might even be better than, those based upon interoccupational differences.

Myers & Errett (53) raise an interesting question concerning the problem of preselection in weighted application blank studies. Their search of the literature failed to reveal a single study giving attention to the amount of preselection existing before weighting is based on criterion groups. In a study of 291 clerical people, they present results showing that of the 19 biographical items being validated, 10 were already being used by interviewers as a basis of preselection, with a differentiating significance beyond the .001 level of significance. Myers & Errett present three suggestions for ways in which preselection weights might be used along with the usual weights.

Several studies having to do with psychological job-requirements analysis are of interest. Boling & Fine (8) report a study of temperament requirements based upon job descriptions for a number of Civil Service positions. When these were put in terms of traits there was low reliability as to inter-rater agreement of the patterns required for a given job. However, when these requirements were reformulated as situations in jobs requiring some common adjustment of workers (i.e., defined by an over-all situational context rather than a clinical concept) reliability was greatly improved. Implications for more adequate criteria in measuring personality and job adjustment seem obvious. In a somewhat different vein, Dunnette & Kirchner (16) made an analysis of self-ratings of 35 activities by a group of retail salesmen and a group of industrial salesmen. They found a different pattern of predictor variables for these two groups in measures of intelligence, interest, and personality, including the Strong and the Edwards Personal Preference Schedule. The over-all validities were not impressive, but in the right direction when the pattern of scores which will predict among these two groups is related to the differences and their check lists of activities. In other words, the differences among measures predicting success in these two groups were well in line with the differences in the tasks and activities indicated by the check-list analysis.

The implication of Dunnette & Kirchner's study is that more refined analysis of criterion measures may lead to better and more differential predictions. Such concerns are expressed in the various papers presented in a symposium on "The Development Processes for Indirect or Synthetic Validity" authored by Balma *et al.* (3). While these papers, particularly those by McCormick, by Primoff, and by Griffin, indicate reasonably good success in a start on better defining simple jobs, none of these writers has attempted to come to grips with the more complex problems involved in this

approach at the higher levels of supervision and management. Some provocative leads in this regard were developed in a recent book by Stern, Stein & Bloom (69), but no industrial validation studies following these lines have been published as yet. It appears clear from some of these papers, at least, that what is needed is more intensive study in various situations with a given technique or approach, rather than to have so many of us busy dreaming up new techniques or variations on a theme with little reference to what is being done by our colleagues.

*Nonmanagerial selection.*—Relatively little appeared during the last two or three years which suggested significant advances or contributions in selection at the nonmanagerial levels. The United States Employment Service (USES), presenting its results in the *Validity Information Exchange of Personnel Psychology*, continues to be one of the major publishers of validity studies in a wide variety of occupations. These, of course, pertain to the validation of the General Aptitude Test Battery. About 100 such studies have appeared over a six-year period and represent about one-third of the total number published in *Validity Information Exchange*. There is a need for someone to review and organize these separate studies in order to pinpoint the general conclusions that may be drawn with respect to the validity of the several parts of this battery for various categories of jobs. There is a need, too, for a critical review of the USES's rather unique approach to the statistical aspects of its studies.

Working with a group of textile employees in a variety of jobs, Merenda & Clarke (45) report a follow-up validation of the predictions of untrained interviewers and of blind analysis of Activity Vector Analysis (AVA) profiles. With 30-day ratings as the criterion, the AVA predicted no better than did the interviewers. For the total group (the performance of a portion of which was not predicted by the interviewers), the AVA correlated somewhat higher than the interviewers. But this superiority did not obtain across all criterion raters. No other predictors are reported. Data with respect to the jobs and the predictors used are so scant as to render evaluation of the results difficult.

One important study which appeared in 1959 deserves special mention. This is the report by Thorndike & Hagen (74) of a follow-up study in 1955 and 1956 on 10,000 aviation cadet candidates tested on a 20-variable battery and given a biographical information form in 1943. This review of choice of occupational field and "success" therein is at once an outstanding and unfortunate contribution. By virtue of both prescreening and self-selection, the sample available was admittedly unrepresentative of the age group from which it was drawn. Responses to the follow-up questionnaire were obviously biased. Occupational groups, for the most part, were small and heterogeneous with respect to the nature of duties and the firms in which they were performed, as well as with respect to the geographical area from which drawn. One wonders, after reading the authors' honest exposition

of the biases in their sample and in the inadequacy of the criteria, why they went to so much trouble and expense to conduct the study. They conclude (p. 50),

We should view the long range prediction of occupational success by aptitude tests with a good deal of skepticism and take a very restrained view as to how much can be accomplished in this direction.

This conclusion is based upon tenuous grounds. The study is unfortunate in that statements such as the above have already been extracted from contexts by the public press and presented as proof that aptitude testing in general has no validity. This study, of course, sheds no light on the validity of tests in specific situations with homogeneous jobs and more meaningful criteria.

*Selection at the managerial level.*—Most of the articles appearing during the last two years in this area have to do primarily with discursive analyses of the problem of prediction at the executive level. In a very sober and useful rebuttal to points made by Whyte in *The Organization Man* (78), Stark (68) shows the fallacies of Whyte's arguments and makes a good case for the use of psychological testing in selection of executive personnel. Unfortunately, this is not likely to receive the same publicity as Whyte's tirade. Gellerman (22) presents an analysis of some of the problems in executive placement and lists as crucial issues seven "deadly sins": careless treatment of candidates, overdependence on expert opinion, overestimating or misjudging job requirements, making stop-gap appointments, "pigeon-holing" prospective candidates, disregarding the company personality, overlooking personal compatibility. All of these points deserve serious attention on the part of those doing individual assessments, for they form the nucleus of what must stand as the criteria for a prediction decision in matching a given individual with a particular job. In a similar vein, Davies (15) discusses the need to be more alert to the unusual combination of qualities needed for success in research managers, suggesting that somewhat more attention to the definition of criteria of success in a research manager might lead to better prediction. Taylor (71) reviewed the history of executive research and indicates needs in this area if the problems of executive selection are to be solved.

A fairly good review of the field of differential testing for high-level personnel is given by Michael (47). This sums up reasonably well the status of where we are currently and gives some suggestions for future emphasis in research at this level. Taylor & Nevis (72) make a plea for more research, particularly using projective techniques in a more sophisticated way than has been attempted previously in the field of management selection.

Three studies which presented validation of one type or another are worth mentioning. Ghiselli (25) compared the differences in self-concepts



among top management, middle management, line supervisors, and line workers on his self-description inventory. His results, based on about 800 men, support what every practitioner in this field has found on the basis of his day-to-day experience, namely: top and middle management people are about equal in intelligence but differ in significant ways as to drive and personality characteristics. In another rather ingenious study based on 63 Italian Civil Service Administrators, Ghiselli (24) identifies a measure of individuality as a factor in the success of management personnel. It is interesting, in this regard, that here his criteria were peer ratings of job performance. The obtained correlation of .54 suggests that, at least as peers see it, the more individualistic, less conforming manager is considered to be a better man. It would have been interesting in this connection to see what relationship between the individuality measure and ratings by top management would yield, for it is here that the issue of conformity and job success becomes more crucial. Mullins (50) presents a study in which an interest questionnaire, a vocabulary test, and nine tests of the Guilford Creativity Battery were administered to 131 research physical scientists. Of 42 test scores derived from the battery, four were significantly related to a criterion of over-all rating, and seven to a criterion of publication frequency. While such a finding is promising, this study presents no real light on the difficult question of understanding the creative process or that of judging creative performance.

In a three-year follow-up of more than 500 insurance salesmen, Merenda & Clarke (46) validated the four Activity Vector Analysis (AVA) factors and several isolated personal history items against a succeed-fail criterion. The highest biserial for the AVA factors was .18. The highest single personal history item also had a validity of .18. Obviously, the multi-item Life Insurance Sales Aptitude Test would again do a better prediction job than the AVA as indicated in the earlier study by Wallace, Clarke & Dry (77).

From Sweden comes a rather unique study by Trankell (75), who reports one of the few validation studies free of criterion contamination. Working on the selection of Scandinavian Airline System's flight personnel, he compared the validity of psychological tests per se with the validity of psychologists using the tests in conjunction with interviews. Generally, the psychologists augmented the validity of the tests by about .20 correlation points.

Operating in a similar but more complicated situation, Barrett (4), Prien (62), and Huse (33) studied various aspects of the Personnel Audit Program at Western Reserve University. If we consider the fact that the interviews in this program were generally conducted by graduate students, many of whom had no industrial experience, and that audits were performed for a variety of clients and an even greater number of jobs, the findings from these studies indicate the multifaceted professional assessment to be a

promising selection tool. Much more research, however, is needed in this area.

#### PERSONNEL SELECTION IN THE ARMED FORCES

*Enlisted personnel.*—The authorized strength of the three major branches of the military constitutes somewhat less than 4 per cent of the country's total work force. While it is difficult, if not impossible, to compile comparative figures with respect to the level of research activity in personnel selection in the military as against industry, perusal of the literature and familiarity with personnel in both areas lead one to the inescapable conclusion that the Armed Forces are at least a decade, if not more, beyond their civilian counterparts in terms of personnel research in general, and personnel selection in particular. Except for a few extremely large industrial organizations such as General Motors, General Electric, Bell Telephone, Prudential Life Insurance Company, Standard Oil of New Jersey, etc., relatively few businesses employ psychologists in a purely personnel research capacity. What research the industrial psychologist can "get away with" is usually the by-product of an operational assignment.

In addition to these, the Air Force and Navy sponsor a modest program of contract research with a variety of civilian organizations. Under the category of Personnel Psychology, the National Science Foundation's semi-annual report of government-sponsored research projects in psychology, psychiatry, and closely related areas (54) lists 49 separate research contracts, all but seven of which are sponsored by one or another of the military agencies. Interestingly enough, none of the research in these areas, nor in the training field, is under National Science Foundation sponsorship, nor does any contract research appear under the sponsorship of the Personnel Research Branch of the Adjutant General's Office. Most of the Air Force research is under its Personnel Laboratory auspices, while Naval studies are primarily undertaken through the Personnel and Training Branch of the Office of Naval Research. The latter organization, to a large degree, comes closest to filling in the vast void of basic research in the personnel selection area. It underwrites such studies as French's "Problems in Psychological Test Development," Tiedman's "Occupational Ability Patterns in Personnel Classification," and Horst's "Techniques in Differential Prediction" (32). Except for such isolated studies, however, really basic research in the personnel selection area appears to be considered too "applied" for National Science Foundation sponsorship and too "long hair" for sponsorship by industrial organizations or other military agencies. Unfortunately, this situation has reinforced the absence of new methods, techniques, and approaches to the personnel selection area by creating a kind of no-man's land, and deserves mention as crucial comment on the status of recent research.

Since its fundamental objectives are to predict on-the-job performance,

personnel selection in the military has much in common with its civilian counterpart. Yet it is not without several unique features. During periods of conscription the military is faced with "job applicants" whose motivation is primarily to be rejected. In peace-time periods the Navy and Air Force are generally able to fill their quotas through enlistments while the Army is dependent, in part at least, upon the draft. The initial psychological screening device is the Armed Forces' Qualification Test (AFQT), jointly developed by all three services and employed by all. The development of the most recent forms of this primary mental screening test is described by Mundy, Burke & Bayroff (51). The AFQT was designed to fail the lowest 10 per cent of the total population, but is administered to only a selected portion thereof. To check the adequacy of the instrument, Merck & McMahon (43), through an elaborate manipulation of a variety of official statistics, determined that a total of 10.9 per cent of the total male population between the ages of 18 and 34 could be expected to fail the test. To solve the problem of the malingerer, the Department of the Army (52) has developed a failure key by comparing the responses to easy verbal and numerical items of men truly illiterate and of those faking illiteracy. These keys differentiate between true and experimentally deliberate failure. Such an approach may have some possibilities for industrial use in differentiating between low intelligence and anxiety-induced poor test performance by persons of otherwise presumably good intellect.

In the Armed Forces the real personnel selection task occurs after the individual has been "hired," i.e., inducted into the service. It is after induction that each of the services administers its basic classification battery and becomes confronted with the complicated problem of differential prediction. An industrial organization generally hires new employees only when vacancies occur and for specific positions. Except under special circumstances, the civilian personnel selected are expected to have the basic skills and knowledges demanded by the position. The military, on the other hand, inducts and affords basic training to a pre-established number of individuals. Inductees are generally young, inexperienced, and untrained in most of the military specialties which require their services. Consequently, the military has a much more extensive training problem than does civilian industry. Assignment to training must be made upon the completion of basic training in accord with quotas of available vacancies. Under these circumstances it frequently happens that the training vacancy for which a man is best suited by prior experience, inclination, and aptitude may not be available, and a second, third, or even fourth choice from both the man's and an optimum classification point of view must be made. It is these circumstances that make it erroneously appear that the military is cavalier and sometimes even stupid in the placement of individual enlisted men.

Induction into one of the military services involves a much greater

change for most people than does taking a civilian job. Not all individuals can adjust to this change, and those who cannot create quite a costly problem. Considerable research has been done, and a variety of devices has been used experimentally, in an endeavor to predict failure to adjust. Benton & Bechtoldt (7) tried unsuccessfully to construct keys to a vocational interest questionnaire as predictors of personal adjustment in the Navy. Somewhat greater success was realized by Gunderson, Ballard & Hoge (28) using a combination of personal background and aptitude score variables along with a delinquency potential scale, while Locke (38) did a *post hoc* study of 50 incarcerated offenders and a matched group of 50 sailors in nondisciplinary status, using the Stein Sentence Completion Test. Blind analysis of the tests indicated that the offenders were more mal-adjusted and showed significant differences in attitude toward inferiors and peers and in the possession of guilt feelings and in energy level, than the control group. Ballard, McHenry & Grant (2) report the four-year follow-up of a pencil and paper attitude scale in the prediction of successful retraining of court-martialled Naval and Marine personnel who entered a retraining command after having served their sentences. The instrument proved moderately successful and the authors suggest that its predicted efficiency could be increased by the addition of other variables.

Williams & Zimmerman (80) report that the conventional psychiatric screening at the time of induction validly predicts whether or not a recruit would still be on active duty six months after his induction into the service. Flying-induced anxieties were found to be significantly discriminated by a self-rating reported by Worchel (81).

Morton *et al.* (48) report that two nonlanguage tests and a verbal test administered to more than 2000 enlisted men of limited ability failed to predict military performance ratings and the type or level of duty assignment, whereas Carleton *et al.* (13) obtained hold-out sample validation of between .25 and .31 against a military adjustment criteria on a sample of 1500 enlisted men with the Army Personality Inventory, adapted from the Minnesota Multiphasic Personality Inventory.

An even more difficult problem with which the military is confronted is that of predicting combat effectiveness. This is, of course, a performance area for which no real criterion exists during peace time. The three-volume publication by Ginzberg and associates (26) looks at the problem in retrospect, examining in turn the magnitude of the problem of ineffectual performance in relation to military, particularly Army, practices and policies during World War II. Individual cases, their antecedents and consequences, are examined in the second volume, and an endeavor to evolve a solution to the situation is presented in the third. Little was done in this area during World War II, but both the Army and the Air Force endeavored to collect pertinent data during the Korean War. The research conducted by the Army is reported by Willemin & Karcher (79). It

culminated in the development of interest and personality tests which, combined with arithmetic reasoning, were shown to have a validity of .32 for combat effectiveness for infantry assignment. Trites & Sells (76) report on the validity of precombat predictors of both objective and rated measures of combat success in a small group ( $N$  less than 40) of pilots. Validities are generally low and frequently negative, but there is consistency between combat effectiveness as rated by superiors and peers and by psychologists.

Egbert *et al.* (18) report that a battery of tests administered to Korean service men distinguished "fighters" from "non-fighters." Leadership and masculinity measures are reported as the most discriminating characteristics. On the basis of their results they consider the prediction of combat effectiveness to be a possibility.

But combat is not the only area in which selection must be made in a vacuum. As we embark upon the conquest of space, the personnel to man the space ships will be required. Gerathewahl (23) explores the problems and possibilities in this area.

The most frequently appearing research reported in the military is validation of classification tests against military course grades and, to a lesser degree, against on-the-job criteria. In a sense, the latter may be somewhat confusing, since in the case of both the Navy and the Air Force these generally (but with some exceptions) consist of scores upon written proficiency tests. Summary reports cover much of the research done in this area. The Bureau of Naval Personnel Research Report 57-1 (11) presents expectancy tables and validity coefficients from 74 studies in which various combinations of the tests in the Navy Classification Battery were validated against final school grades.  $N$ s varied from a low of 72 to a high of 1325. The 74 studies cover 46 different curricula, the duplication resulting from doing separate validation studies where the same course was presented at more than one training school. Generally, there is a high degree of consistency in the magnitude of the correlations in studies where replication took place. The arithmetic and mechanical tests, for example, were validated in the Aviation Machinist's Mate course at two schools with  $N$ s of 1325 and 1283 respectively. The corresponding validity coefficients were .58 and .52. The General Classification Test (GCT) and arithmetic tests, validated against final grades in the Electrician's Mate School at three different locations with  $N$ s of 677, 1181, and 1234, yielded validities of .72, .53, and .47 respectively. Somewhat greater variability in validity is found in the yeoman classification, where GCT and clerical validities varied from .13 to .54. The range of the 74 correlation coefficients presented was from .01 (Mechanical tests against Torpedo Men's Mates course grades  $N$  equals 734) to .80 for GCT and arithmetic on a sample of 1173 airmen. The medium coefficient is in the low .50s, and only eight of the validities are below .31.

Unfortunately, this study reported the correlation of course grades with only those tests in the Navy battery which, in each case, determined the seaman's eligibility to attend the course. Thus, no opportunity is afforded to examine the extent to which differential classification is successfully operating in the Navy.

Unlike the Navy, which apparently selects and weights individual tests in accordance with the assumed or empirically determined requirements of the course to which men are to be assigned, the Army and Air Force have constituted Aptitude Indices and Aptitude Areas, respectively, which are standard combinations of selected tests. A summary report by Brokaw (10a) presents the validity of each of the five Aptitude Indices against final school grade and against Airman Proficiency Tests. In 12 of 13 military specialties in the Mechanical Career Field, either the General or Electronic Aptitude Index is a better predictor of school grades than is the Mechanical. In only six of the 10 specialties in the Administrative Field is the Administrative Aptitude Index more valid against final school grades than the General or Electronic Index. The General Aptitude Index is the most valid in seven of seven specialties in its own field, while in the electronic area, 12 of 14 studies show the Electronics Aptitude Index to be the most valid against school achievement criteria. In 20 additional studies from various areas, only four showed the greatest validity in the career field to which it belonged. Of these four, two were in the electronic area. Generally, the same statement can be made with respect to the validities of the Basic Classification Battery against the Airman Proficiency Tests. It is doubtful, however, that the differences among the validity coefficients for a given specialty against either the final school grade or Airman Proficiency Test criteria, respectively, are statistically significant. Rather, it would appear that the general order of magnitude of the validities for all aptitude indices varies with the particular military occupational specialty. We would venture to guess that this phenomenon is, in part at least, a function of the extent to which the variance in the final school grade criterion is accounted for by pencil and paper tests similar in nature to those in the Basic Classification Battery. The correlations between final school grades and Airman Proficiency Test range from .27 to .80.

In an earlier report by the same author (10), the Armed Forces Qualification Test (AFQT) was validated against the same two types of criteria for what appears to have been the same population. Validities for this much shorter test range from a low of .13 to .66 against final school grades and from .07 to .55 against proficiency tests. None of the correlations in either study appears to have been corrected for restriction in range. That substantial restriction occurred is evidenced by the fact that the AFQT mean is reported to be near 50 with a standard deviation of about 22 for the total group, but varies widely among the several subgroups. The group having the lowest mean was parachute riggers with 39.31 and a standard deviation



of 13.68. At the other end of the scale, general instructors had a mean of 82.60 with a standard deviation of 14.00.

Leczmar (36) reports the preparation of a shorter (four hour) Airman Classification Test, presumably to replace the Airman Classification Battery AC2A. The Radio Aptitude Index was to be dropped, thereby eliminating the Aural Code Tests. Several of the spatial tests in the older battery were also to be deleted and four of the tests shortened in length by dropping some of the less discriminating and easier items. The purpose of a shorter new test is to enable its administration at recruiting centers in order to make selective enlistment possible. The new program was scheduled to go into effect January 1, 1960.

No summary of validities for Army studies was located by these reviewers. The Personnel Research Branch of the Adjutant General's Office (59, 60, 61), however, published a series of 36 studies in the Validity Information Exchange of *Personnel Psychology* covering as many military occupational specialties. A unique feature of these studies was that they incorporated validation of the same aptitude areas and Army Classification Battery Tests against school and on-the-job criteria. The on-the-job criteria consisted of a combination of supervisory and peer ratings. Validities were given for the appropriate and next nearly appropriate Aptitude Area, and for each of the 10 tests in the Army's battery. Generally, there were considerably more cases in the school than in the on-the-job sample, but both were substantial. Validities against course grades were comparable to those found in the Navy and Air Force, frequently in the .50 to .60 range, and sometimes going as high as .86.

Comparable validities against the on-the-job rating criteria, however, were strikingly and substantially lower. In the case of teletypewriter repairmen, to take a horrible example, the Electronic Aptitude Area correlated .64 with the school grades and -.05 with on-the-job ratings. (The *Ns* were 903 and 178 respectively.) For the individual tests the lowest correlation with school grades was .39 (with Army Radio Code), while for the rating criteria the highest correlation was .16 (for Army Clerical Speed). That the results were not always so dramatically different is evidenced by the findings with respect to the Personnel Specialist category, with 572 and 185 cases respectively against the school and on-the-job criteria. The Clerical Aptitude Area had a validity of .63 against the school criteria and .40 against the job criteria, while the validities of the General Technical Aptitude Area were .69 and .42 respectively. Validities of the individual tests against school grades ranged from .24 for Automotive Information to .66 for Arithmetic Reasoning, while validities against the job criteria ran from .12 to .44 for the same two tests.

Merenda (44) studied the validity of Navy proficiency promotion exams against ratings by superior officers for 40 different jobs and job levels. The number of cases ranged from 28 to 245 and the validities from -.15 to .70.

Generally, it was observed that the examinations for higher level petty officers were more valid than were those for lower level positions.

While many military occupational specialties have no civilian counterparts, there are a large number of functions in the Armed Forces similar, if not identical, to those performed in business and industry. Certainly, insofar as aptitude tests are concerned, no great difference exists between those used in the military and those available to the civilian personnel man. McReynolds (42) presents the intercorrelations of the tests in the United States Employment Service General Aptitude Test Battery and the Airman Classification Battery for a sample of 2649 basic airmen. Correlations between equivalent tests range as high as .74. Thus, it would appear that these tests are about as closely related to their civilian counterparts as are any pair of instruments not especially created to be equivalent.

As in civilian life, then, the prediction of achievement in course grades by aptitude tests is rather readily predictable. Validities against bona fide on-the-job criteria are of a much lower order of magnitude. Here, as in industry, the ever-present problem of criteria remains unsolved. Two long-range research projects under Office of Naval Research sponsorship are approaching the problem from different angles. Siegel & Benson (65) and Siegel, Schultz & Benson (66) have been endeavoring to develop technical performance check-list criteria which are scalable in both a Thurstone and Guttman sense. These studies of aviation electronics technicians and aviation machinist's mates, respectively, do not demonstrate these criteria to be highly predictable by either an attitude scale or aptitude test. Mackie & High (39) have approached the problem through performance tests and supervisory ratings with only slightly greater success on a sample of Navy machinery repairmen. Three of eight aptitude tests, Mechanical Knowledge, Spatial Relations, and Perceptual Speed, correlate significantly with a job performance test and yield a multiple  $R$  of .39. Supervisory ratings correlate significantly only with Mechanical Knowledge. Five predictors, four of them school performance test variables, yield a shrunken multiple  $R$  of .58 with a composite criterion consisting of job performance and supervisory ratings.

*Officers.*—The problems of officer selection are, if anything, more difficult than are those at the enlisted level. A portion of the officers in all three services is, of course, obtained through the respective academies, but officer candidate schools and ROTC account for a far greater proportion. Much research has been accomplished over the past years in the prediction of success in the various officer training organizations. Parrish & Drucker (58) summarize the Army's 16 years of research in the development of instruments for the selection of officer candidates. It has met with about the same level of success as has academic prediction in other areas, but efforts to predict on-the-job performance of officers appear to have been singularly unsuccessful. Two studies merit mention in this connection. Thorndike &

Hagen (73) collected follow-up criterion data on 873 Air Force officers to whom aptitude tests had been administered some 12 years previously. Twenty-three predictors were validated against 13 criteria. Examination of the 299 validity coefficients strongly suggests that their distribution does not differ substantially from chance. The highest correlation is between a mathematics test and the educational level reached by the officer. None of the predictors correlates with the average efficiency report, nor with present rank. While one might readily question the validity of the criteria themselves, they are nonetheless operational in nature and a reflection of how the individuals in the study were evaluated by their respective superiors, as well as the progress they have made in the service. For those who remained in the Air Force, these criteria are evidently not predictable by aptitude tests.

An elaborate study of Air Force captains, begun in 1952, is reported by MacKinnon (40). A three-day battery of tests had been administered to more than 300 captains stationed at various Air Force bases. In addition, 100 of these were sent (10 at a time) to the Institute of Personality Assessment and Research (IPAR) at the University of California for a "living-in" assessment. In the field testing, 27 tests yielding 233 predictor variables were obtained. An additional 398 predictor variables were generated on the 100 officers in the living-in phase, making a total of 631 predictors. With almost twice as many variables as subjects, one might ask how many degrees of freedom remained in the statistical analysis. The final report maintains that the ultimate criterion of the study will be the level and quality of leadership attained by the officers 10 to 15 years hence, and "the final test of the validity of our 631 predictive measures cannot be made until that ultimate criterion is at hand." For interim criteria the study employed: (a) the regular officer effectiveness report over-all evaluations, (b) promotion board ratings, (c) special ratings obtained by superiors on general effectiveness, interpersonal relations, and conscientiousness, and (d) "job concept interview ratings" obtained in extensive interviews by the staff of the senior reviewer under separate contract.

The highest correlation obtained was one of .59 against a criterion index which consisted of a combination of officer efficiency reports, promotion board ratings, and superior officer ratings. The predictor in this case was the military officer performance scale of the Air Force Preference Inventory. The military officer effectiveness scale of the IPAR questionnaire correlated .49 with the same criterion index. Most of the high correlations were between the job concept interview and the living-in assessment variables, which indicates that evaluations made by psychologists tend to agree with evaluations made by other psychologists, regardless of the technique employed.

If the job concept interview criteria are omitted from consideration, it is doubtful that the distribution of the remaining validities differs signifi-

cantly from chance. It is patent that it should be easier to predict temporally close than remote criteria. The expectations that the predictors will gain validity with the passage of the years seems to the reviewers to be a rather futile one. Rather, it would seem that this is another case in which the research design was so concerned with the development and administration of predictors (and certainly well concerned) that it afforded inadequate attention to the development of criteria of job performance. On the other hand, if one accepts the notion that psychologists can judge job performance better than supervisors, the validities against job concept ratings are impressive.

Toward the end of World War II there appeared to be a great deal of concern with the criterion problem. Perhaps failure to have solved it successfully may have led the profession to return to the earlier practice of affording it inadequate attention. Certainly, publication emphasis in the military, as well as civilian, studies has returned almost exclusively to concern with predictors. There appears to be a need to revitalize the performance measurement area and afford it the research attention it requires.

#### THE SUMMING UP

A fairly common fallacy among businessmen is the belief that the administration of aptitude, proficiency, or personality tests, by itself, constitutes "scientific selection." In a recent survey of more than 200 companies, made by the *Industrial Relations News* (34), it was reported that 65 per cent were using formal testing programs for job applicants. The *Industrial Relations News* reports,

Among panelists using tests, opinion was unanimous concerning their worth. Even among non-users, a flat sixty per cent indicated they also thought tests useful, while only two per cent considered them of little value. About thirty-eight per cent of the companies not using tests expressed no opinion on their usefulness.

To the extent that this sample is representative of industry as a whole, it should be apparent that the use of psychological tests as selection devices far exceeds the availability of trained personnel competent to install and interpret them. The growth of the "do-it-yourself" selection testing by untrained or partially trained amateurs has undoubtedly been encouraged by the ease with which tests, particularly aptitude tests, have been made available to all who wish to purchase them. Even the most reputable of test publishers have lowered or removed the barriers that previously restricted the distribution particularly of aptitude and intelligence tests. In its 1960 catalog, The Psychological Corporation (63) devotes two pages to "Suggestions for Personnel Managers." The lead paragraph of these suggestions reads,

The purpose of these pages is to call the personnel officer's attention to the tests listed in this catalog most commonly used for employment purposes. These tests, published by The Psychological Corporation, can be easily administered and scored

in the personnel office and are available to all business and industrial firms (emphasis ours) using tests in the selection of new employees and the upgrading of personnel within an organization.

Nowhere in the ensuing paragraphs, covering five categories of positions for which tests are recommended, does there appear a single word of caution regarding possible misuse of the suggested instruments, nor even a hint of the need for validation. The Psychological Corporation is not singled out for criticism as being an isolated offender in this regard. Rather, the above is quoted to illustrate the extent to which even this highly ethical test publisher has gone in making some of its instruments available on an almost unrestricted basis. Other publishers, in spite of sometimes pious statements made with respect to the qualifications required of test purchasers, are often even freer in their distribution policies than is The Psychological Corporation. In spite of the well-established specificity of validity, test manuals frequently make pat specific recommendations with respect to the tests that should be used for a given *job title* without any knowledge of the specific requirements, or more particularly, of the applicant population. In at least two cases the test manuals recommend specific cutting scores for selected positions. These are in the manuals of the tests published by Industrial Psychology, Inc. and in the manual of the Employee Aptitude Survey published by Psychological Services, Inc. It is interesting to note, in connection with these batteries, that *The Fifth Mental Measurements Yearbook* (12) does not list a single publication in connection with either of these batteries. This is understandable in the case of the Employee Aptitude Survey, which was constructed between 1952 and 1957, but difficult to understand in the case of the tests published by Industrial Psychology, Inc., which have been on the market since 1947.

Referring to Paragraph B of Principle 15 of the American Psychological Association Code of Ethics, which deals with what test manuals should report about the limitations of a test, Holtzman (31) says,

When taken very seriously, I dare say that this principle is violated more flagrantly than any other in our Code of Ethics. The enthusiastic author and obliging publisher cannot resist the temptation to sell the test as the latest word, the answer to every eager personnel executive's problems. In glancing casually through some of the test blurbs currently in circulation, I have been greatly disturbed by the rather cavalier treatment of this fundamental issue by some publishers and their authors. And yet, no one seems to be doing much about it, probably because in most cases there is not much that can be done. Such promotional activities rarely seem outrageous enough to goad someone into pressing charges of unethical conduct.

This inadequacy of test manuals, coupled with their availability to uncritically gullible executives, cannot do other than lead to a rather broad misuse of tests in industry. As Stagner (67) points out,

In the field of engineering, chemistry, power sources, and raw material supply, the

average businessman has learned to think realistically and to demand quantitative evidence concerning the value of an item before buying it. In the novel field of psychological measurement, on the other hand, many executives are still amazingly gullible. They often purchase expensive "employee selection" programs with no scientific evidence that the service offered has any value whatever.

Insofar as the current state of personnel selection in industry is concerned, these reviewers feel justified in concluding that too many untested tests are too readily available to too many unqualified testers. Little real research is being conducted with respect to personnel selection in industry, and almost none on devices other than aptitude and personality tests.

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## INDUSTRIAL SOCIAL PSYCHOLOGY<sup>1</sup>

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The past chapters in *Annual Reviews of Psychology* dealing with the field of industrial psychology have generally included a section or sections on such topics as leadership, employee morale, communication, and employee attitudes. Pursuant to the decision of the Editorial Committee to subdivide certain topic areas further, this chapter will deal exclusively with the social psychological aspects of industrial psychology.

The term industrial social psychology will be used in this chapter to refer to the application of social psychological concepts and theories to industrial organizations. Reflecting the fact that social psychology has been concerned with the explanation of both individual and group behavior, research in industrial social psychology has been based on two general types of models. The first treats the individual as the unit of analysis and is primarily concerned with the effects of the social environment of the work situation on his attitudes, motivation, and behavior. Implicit is the Lewinian postulate that behavior is a function of person and environment. Although most research has been concerned only with the effects of environment on behavior, some attention has been paid to interactions between personality and environmental variables.

The second kind of conceptual model treats the social system as the unit of analysis. It is concerned with the structure and functioning of systems within industrial organizations ranging in size from the dyadic supervisor-subordinate relationship, through the primary work group to the large-scale organization. Problems considered include the conditions under which various types of structures are likely to occur and the consequences of these structures for the continued existence and productive efficiency of the organization.

A few comments on the organization of this review seem appropriate. We will begin with some general comments pertaining to the major developments and trends within the over-all field for the period covered by the review. Then, we will discuss research relevant to the topics of communication, leadership, organizational problem solving and decision making, the motivational effects of task variables, and organizational structure.

This review is based on the period from January, 1958, to March, 1960. In selecting works for the review the authors did not attempt to be comprehensive, but rather decided to limit themselves to a coverage of those

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books, monographs, and articles that seemed most significant and most easily included within the topical framework.

Not all of the studies described were carried out in industrial settings. Much of the research utilizing small groups as well as research conducted in other kinds of social organizations (e.g., the military, hospitals, unions) is extremely relevant to an understanding of behavior in the industrial situation. An arbitrary division between research conducted in industry, on one hand, and other kinds of social organizations, on the other, overlooks the obvious similarity between kinds of problems studied and findings obtained.

#### GENERAL COMMENTS

Haire (32, 33) has discussed three traditional subfields within industrial psychology—(a) personnel psychology, concerned primarily with individual differences; (b) human engineering, concerned with the effects of task variables on performance; and (c) industrial social psychology, concerned primarily with social structure and social motivation. In his description of the latter area Haire goes beyond his previous work on the subject (29) and presents a succinct and thought-provoking analysis of many of the central concepts and problems in the field. Surprisingly enough, however, there is only passing reference to the large amount of research on leadership.

Three recent books, *Organizations* by March & Simon (72), *Managerial Psychology* by Leavitt (49), and *Modern Organizational Theory*, edited by Haire (31), represent important contributions to the field. March & Simon have attempted to integrate within a single framework the approaches of the various disciplines that have been concerned with organizations. Their discussion treats over 200 variables and an even larger number of propositions concerning organizational behavior. These variables and propositions are based on the work of Taylor and his associates on scientific management and the "classical" organization theorists such as Gulick and Urwick; the writings of Merton, Selznick, and Gouldner on bureaucracy; the research of social psychologists on leadership, employee morale, and organizational effectiveness; and the work by March & Simon and their colleagues on organizational problem solving and decision making.

This extremely ambitious undertaking by March & Simon, perhaps, can be viewed best as a springboard for further theoretical development and research. The task of picking up where March & Simon left off is indeed a challenging one and promises to contribute greatly to our understanding of industrial as well as other types of organizations. The fact that March & Simon have reduced the efforts of writers from a number of disciplines to a series of propositions in more or less testable form should not only stimulate research necessary to test the propositions, but also lead to the development of theories capable of more parsimonious treatment of existing data.

Intended primarily for managers, Leavitt's book applies psychological and social psychological concepts and processes to human problems within business organizations. Beginning with the behavior of the individual in the organization, the author examines, in succession, problems of interpersonal relations, small group processes, and organizational structure. This volume provides the reader with a nontechnical introduction to much of the subject matter of industrial social psychology; it is both highly readable and well grounded in theory.

The third volume, edited by Haire, illustrates the diversity in the kinds of organizational phenomena currently being studied. The book is a collection of papers presented at a symposium held by the Foundation for Research on Human Behavior in February, 1959, and represents the thinking of leading psychologists, sociologists, mathematicians, and economists about organizational behavior. Of greatest interest to social psychologists will be Likert's statement of his modified theory of management (55), Haire's application of a biological model to the problem of organizational growth (30), Cartwright's description of the utility of graph theory in the study of organizations (11), and the contributions of Rapoport (85) and Cyert & March (15) to a theory of organizational decision making.

As represented in the Haire book, the last few years have seen much greater variety in the kinds of organizational phenomena studied by social scientists. New problems are being tackled and old problems studied with new perspective. Similarities in interests of the industrial social psychologist and his colleagues in labor economics, industrial sociology, and engineering psychology are leading to greater exchange of ideas and interdisciplinary research.

In addition to an increase in the breadth of phenomena studied, theory and research in industrial social psychology during the last two years suggest a greater recognition of the complexities of organizational behavior. Attention is turning from simple cause and effect relationships between variables to interactions between variables, to mediating processes, and to the fact that changes at one point in the system may bring about related changes in many variables throughout the system.

There is some indication that methodological developments are not keeping pace with theoretical advances. There is still an unfortunate tendency for researchers to rely on *ad hoc* measures of variables. Measures are frequently designed for the particular study in which they are to be used, and evidence of reliability and validity is often not obtained. Some standardization of measures is important to further progress in the field.

Another methodological problem stems from the fact that most industrial studies are carried out within a single organization. Data are obtained on two or more variables, relationships are determined, and the significance of relationships ascertained using standard statistical tests. Strictly speaking, generalizations can be drawn only about the particular organization



studied. The sample is, in no sense, representative of American workers in general or even of workers within a particular industry. Furthermore, the subjects typically represent a highly clustered sample of the population of workers in their own organization. Work groups or departments are selected from a large number of such units and data collected from individuals within these units. As Kish (45) has pointed out, individuals within departments frequently resemble one another with respect to the characteristics being studied and, if so, are not independent sources of data. The use of standard statistical tests and confidence intervals for such clustered samples can result in serious overestimation of the statistical significance of relationships.

One promising approach to these problems involves a broader definition of the population. In a number of recent studies [Argyle, Gardner & Cioffi (1); Dent (17); Mann & Georgopoulos (66)], data have been collected from a number of organizations. It seems likely that increased acceptance of organizational research and the development of research institutes with facilities for large-scale data collection and processing may result in a greater number of multiorganizational studies in the future. Other approaches, involving the statistical treatment of data in order to deal specifically with the "clustering" problem, have been described by Kish.

#### COMMUNICATION

As Redfield (87) has indicated, management's interest in problems of communication in industry has increased rapidly. Ten years ago the subject was hardly mentioned in textbooks on management and business administration. Today, possibly because of the marked increase in size of many organizations and attendant communication problems, the topic of communication has taken its place among the most popular management subjects in America and throughout the western world.

During this period there has also been a great change in the amount and nature of communication research. Earlier attention paid by psychologists to the physical properties of communication stimuli such as size of type and readability of material has been supplemented by research on some of the social psychological and motivational aspects of the process. The recent literature (1947 to 1957) on communication in business and industry has been summarized by Sexton & Staudt (96).

One of the central questions to which research has been directed concerns the relationship between communication and effective group functioning. Discussions of "adequate" or "effective" communication beg the question of the consequences of the communication of information of various kinds for the effectiveness of the system. Not all information is equally relevant to decision making, the changing of attitudes or motives, the removal of barriers to goal attainment, or other organizational consequences. Indeed, it seems likely that communication of some kinds of information

either upward or downward in the organization may be dysfunctional. Schutz (95) suggests some situations in which problems are likely to result from "too adequate communication," while Maier (59) hypothesizes that the communication of superiors' evaluations of subordinates' role performance may arouse defensiveness on the part of the subordinate.

Kahn (42) reports findings from a study of workers' productivity in a tractor factory which bears on the relationship of communication between superior and subordinates to productivity. All employees who worked on jobs for which time-study had established a rate were asked to estimate what they considered to be "reasonable productivity" on their job. The foremen of these workers were also asked to estimate what the men would consider to be reasonable productivity. The foremen consistently overestimated the men's productivity standards; moreover, those who were least accurate were least successful in achieving high productivity. Similarly, Harrison (36) found that workers who can accurately predict what their supervisor expects of them are rated higher in job performance.

Social scientists have also been concerned with the determinants of communication in organizations, i.e., with the conditions under which information will be transmitted and received by persons or parts of the organization. On the basis of interviews conducted in the spinning department of a synthetic textile mill, Simpson (98) hypothesizes a curvilinear relationship between mechanization and communication between supervisors and subordinates. Under conditions of low mechanization instruction must be given by superiors to subordinates, who, in turn, must report their progress up the line. As mechanization increases, however, the work pace becomes controlled by the machine, thereby reducing the need for close supervisory-subordinate communication. However, Simpson suggests that extreme mechanization, as in automation, increases the frequency and seriousness of machine breakdowns and, thus, requires a greater amount of vertical communication.

In a study of communication between high-level managers and their subordinates, Maier *et al.* (64) determined the amount of agreement between subordinates' perceptions of various aspects of their jobs and the perceptions of their jobs by their immediate superior. The results show a generally low level of agreement between the two sets of perceptions. There was least agreement concerning the obstacles or problems confronted by the subordinate and most agreement concerning the duties performed by the subordinate. In a follow-up study, Read (86) found a significant negative relationship between both the strength of the subordinates' promotional desires and amount of promotional mobility and the accuracy with which their superior perceived their job difficulties. Superiors had less understanding of the job problems both of subordinates with strong promotional desires and of those who had moved up rapidly within the organizational hierarchy. Read interprets the findings as reflecting the tendency for persons with

strong promotional ambitions to conceal the fact that they are experiencing problems which might affect their chances for promotion negatively. This interpretation is strengthened by the finding that the size of negative relationships between promotional desire and understanding of job problems is affected by the amount of trust the subordinate has in his superior. The greater the amount of trust, the less the negative correlation between these variables.

Findings by Vroom (114) suggest some ways in which the motives of the receiver of communication may distort his perception of the attitudes and feelings of others. Supervisors were found to overestimate the similarity of subordinates whose work was evaluated positively and to underestimate the similarity of subordinates whose work was evaluated negatively. This tendency was observed for a wide range of characteristics, both job and nonjob related, although it was found to be most marked for those which were central to the self-concept of the supervisor.

Similar results were found in another study of the effects of employees' attitudes toward the organization on their perception of organizational goals (116). Employees who expressed positive attitudes toward the company saw top management as having very similar opinions to their own, and those who expressed negative attitudes toward the company saw top management as having very different opinions. When employees' perceptions concerning top management's beliefs were compared with top management's statements of their own beliefs, employees with positive attitudes toward the company were found to underestimate the degree of agreement. These findings are consistent with the cognitive balance theories of Heider (37), Festinger (20), and others and suggest that information which increases the amount of balance is likely to be remembered and believed, whereas information which decreases the balance is likely to be forgotten or distorted.

Sirota (99) found that employees' understanding of company policies and goals was a function of the degree to which their promotional needs were being satisfied. Employees whose promotional aspirations were moderately frustrated were found to have greatest understanding, while highly frustrated employees had least understanding. Sirota interprets these findings in terms of frustration theory. A moderate amount of promotional frustration might be expected to produce adaptive or problem-solving behavior and motivate the employees to search actively for information concerning the demands and expectations of management. On the other hand, severe promotional frustration would be likely to produce maladaptive or frustration-instigated behavior characterized by a narrowing awareness of the alternative paths around the promotional barrier.

While these studies are suggestive of some of the psychological processes involved in communication in complex organizations, some of them are beset with serious methodological difficulties. Cronbach has recently written an excellent article describing the problems attendant on the use of

a dyadic measure which he defines as one which "compares descriptions of statements about, or actions by two persons" (13, p. 355). Researchers concerned with the communication process would do well to heed Cronbach's suggestion that interpretations of data based on dyadic measures and concepts are justified only if they account for the findings better than interpretations based on the separate components of the measures.

#### LEADERSHIP

Leadership continues to be the most popular topic for research and writing within the field of industrial social psychology. Although there seems to be little agreement concerning the definition of the concept or on the approaches to be used in its study, many social psychologists share, with executives and administrators, an interest in leadership phenomena.

A number of books have appeared on the subject during the period covered by this review [Bass (5); Bellows (8); Browne & Cohn (10); Fiedler (21)]. Bass's work is the longest and, in our opinion, the most noteworthy. He describes a theory of leadership that is systematic and extremely broad in scope. Among the most appealing features of the book are the extensive coverage and the integration of past research (over 1100 items in the bibliography) which provide, for the most part, quite impressive support for the generalizations contained in his theory.

A major difficulty in integrating or evaluating leadership research stems from the fact that this concept has been used in so many ways. The varied conceptualizations are reflected in the use of such divergent criteria of leadership as occupancy of a position of status and authority (93), superior ratings (4, 117), peer ratings (71), observer ratings (71), group productivity or effectiveness (1, 21, 54, 77), subordinate attitudes (66, 110, 117), subordinate absenteeism (1), and quality of joint decisions (62, 65). The extremely broad nature of the phenomena represented by the leadership concept supports the contention of French & Snyder that the term "requires a more limited definition in order to become a useful scientific concept" (24, p. 118).

Bass (5) attempts to clarify the conceptualization of leadership by distinguishing between attempted, successful, and effective leadership, all of which are defined in interpersonal terms. Attempted leadership refers to attempts on the part of A to change B; successful leadership occurs when B's behavior actually changes as a result of A's efforts; effective leadership occurs when the change in B's behavior results in greater satisfaction, reward, or goal attainment on the part of B.

It will be helpful in our discussion of this area to distinguish between efforts to determine the personality traits associated with leadership, efforts to determine the behaviors or methods associated with leadership, efforts to ascertain the role of situational factors in leadership, and efforts to develop leadership.

*Personality traits in leadership.*—Although there are signs that the

"great man" theory of leadership is dwindling in popularity, some efforts continue to be directed toward identifying the personality characteristics of effective leaders. Mann (71) has reviewed the relationship between personality factors and leadership in 115 small-group studies carried out from 1900 through October, 1957. He finds fairly consistent positive but not high relationships between evaluations of leadership and measures of intelligence and adjustment and extroversion. In addition, dominance, masculinity, and interpersonal sensitivity generally are positively related to leadership, whereas conservatism is found to be negatively related. He also obtained some evidence that the relationship between personality factors and leadership varies with the technique of measuring leadership. Adjustment is more consistently related to peer ratings of leadership than to either criterion measures or observer ratings. Extroversion, on the other hand, is not related to peer ratings at all but is consistently related to criterion measures.

Schiller & Abeles (93) constructed a 32-item test of ego-strength and administered it to 109 college seniors. This group was later divided into three categories on the basis of attained leader status. Comparison of the ego-strength scores of the three groups revealed significant differences in the predicted direction. High ego-strength was associated with high leadership status.

*Leadership behavior.*—Whereas small group research has typically been concerned with the distribution of leadership functions among group members, most industrial studies are directed toward determining the effects of the behavior of the formally designated leader or supervisor on group effectiveness. One of the dimensions of leader behavior isolated by the Ohio State factor analytic studies, "consideration," has been related to leadership criteria in a number of recent studies. Generally positive relationships have been found between measures of consideration or conceptually similar variables and group effectiveness. Bass (4) found that supervisors who scored high on the consideration dimension of the Ohio State leadership opinion questionnaire were subsequently rated as higher in effectiveness than those who scored low. Similarly, Likert (54) reports a correlation of .64 between supervisors' attitudes toward their men and measures of productivity of 32 geographically separated work groups, and Argyle, Gardner & Cioffi (1) found a significant positive relationship between the nonpunitive dimension of foreman behavior and productivity in a study carried out in seven British factories. On the negative side, Rambo (84) constructed a new measure of consideration and found no relationship with rankings of supervisory effectiveness.

The extent to which formal leaders involve their subordinates in the decision-making process represents another behavior variable which has received and continues to receive considerable attention. A large number of concepts have been used to refer to this process, including participation in decision making, group decision, consultative management, democratic

leadership, and group-centered leadership. Most of the previous research on the effects of participative leadership has been carried out in the United States. Consequently, there has been considerable speculation about the cultural relativity of these findings. Within the period covered by this review a number of studies were carried out to determine the applicability of democratic leadership in other countries.

French, Israel & As (23) attempted a replication in a Norwegian factory of the original Coch & French (12) experiment in the Harwood Manufacturing Company. Nine four-man groups were changed to producing new products. Four of these groups were changed by the usual methods and were treated as control groups. The other five, as experimental groups, were allowed to participate more in the decisions involved in the change. They met with their foremen and representatives of the planning department to assign each of the five new products to the five experimental groups. Also, two of the experimental groups held additional meetings in which they helped to decide about the division of labor into four jobs, the assignment of these jobs to group members, and the training for these new jobs. The findings indicate no difference between experimental and control groups in level of production. However, experimental groups were found to have more positive attitudes than control groups in 10 out of 14 questions, with three of these differences being significant. The authors interpret the lack of evidence concerning the effects of participation on production as reflecting the low relevance of the decision-making areas to production.

Using Japanese students as subjects, Misumi (77) has conducted a number of experiments on the effects of democratic leadership and group decision on group productivity. In general, democratic leadership was found to be superior to autocratic and laissez-faire leadership, and group decision was more effective than group discussion or lecture methods. There was evidence, however, that the effects of these methods depended to some extent on the nature of the problems used.

In a study carried out in British factories, Argyle, Gardner & Cioffi (1) found less absenteeism in work groups supervised by democratic foremen. There was also a tendency for democratic supervisors to be in charge of more productive work groups, although the latter finding was not significant.

A recent study by Hoffman & Maier (40) suggests that opportunity to participate in decision making may be a more important variable than the actual amount of participation. They conducted a laboratory experiment to determine the applicability of group decision to the distribution of a reward among group members. Students in an undergraduate psychology course were divided into groups consisting of three or four members. Each group was assigned a fixed number of grade points and was asked to decide how these points were to be distributed among individual group members. Since the grade points received were added to those obtained by the student on



course examinations, each person may be assumed to have been highly motivated to attain as many points as he could for himself. All of the groups arrived at a solution, with three-fourths of the groups distributing the points according to need. Satisfaction with the decision on the part of individual group members was found to be related not to the number of points they received nor to the degree of their participation, but to the extent to which they felt they had an opportunity to participate in the discussion and influence the decision.

Maier (58) distinguishes between free and developmental discussion techniques, both of which are consistent with his principles of group decision. In free discussions the leader poses a problem for the group and then conducts the discussion in a permissive manner. In developmental discussion, however, he breaks the problem into parts which are discussed separately before the final decision is made. Maier & Maier (65) have shown that leaders who used the developmental technique produced better decisions than those who used the free discussion technique, and Maier & Hoffman (62) found that the proportion of high quality decisions achieved by developmental discussion could be increased by training leaders.

One persistent problem involved in the identification of effective leader behavior lies in the area of measurement. There is a growing body of evidence indicating great discrepancies between perceptions of leader behavior by different sources. Besco & Lawshe (9) found no relationship between superior and subordinate perceptions of the consideration and initiating structure of the same foremen; Vroom (117) found no relationships between self-reported, peer-reported, subordinate-reported, and superior-reported measures of participation. These results are similar to those obtained previously by Gross (26).

Whyte (122, 123) has discussed difficulties associated with the questionnaire approach to the study of organization and has stressed the necessity of observing behavior and interaction in the work situation. Clearly, the development of methods for the objective measurement of supervisor behavior in field studies appears to be a task of prime importance. Wiridenius (125) has done a systematic methodological study of the time-sampling observation method of measuring the behavior of first-line supervisors in Swedish industry. The purpose of his study was to test hypotheses about factors affecting the reliability and validity of supervisor behavior scores obtained by this method and to determine the influence of observers on supervisor behavior. Interobserver agreement was found to vary between behavioral categories, and a positive relationship was obtained between observers' ratings of the amount of inference required in a behavioral category and the measure of interobserver agreement. Wiridenius attempted to validate his measures by using the supervisor's ratings of his own behavior, obtained by interview, and ratings by the supervisor's immediate superior, obtained by questionnaire. Generally, positive correlations were observed between scores obtained from the time-sampling method and both

of the above criteria with no over-all differences between the magnitude of the relationships for the two criteria. Evidence concerning the amount of observer influence on the supervisor's behavior was obtained by reports from both observers and supervisors. It suggests that influence may occur during the beginning of the study but diminishes with increased contact between observer and supervisor.

O'Neill & Kubany (80) used observations of supervisory behavior in a study of 85 foremen in four production departments of two widely separated automotive assembly plants. Observations were made instantaneously at five-minute intervals for two-hour periods and recorded in terms of two observational categories (functions, e.g., giving information, receiving information, decision making, etc.; and topics, e.g., costs, salvage, and safety). An analysis of the relationship between these observational categories and eight criteria of supervisory effectiveness showed differences which were significant but which did not stand up in cross-validation. The authors conclude that the information obtained by the observation method was not sufficiently novel to justify its use in place of less expensive interview and questionnaire techniques.

In neither of these studies were the observational categories based on theoretical conceptions. The categories were relatively superficial and, in the case of the O'Neill & Kubany study, there was no theoretical rationale for expecting relationships with measures of effectiveness. It is important to observe psychologically meaningful variables, not those that are most obvious and overt. While some sacrifice in reliability of measurement may be involved, the time-sampling methods would seem potentially useful in obtaining relatively unbiased measurements of theoretically important dimensions of supervisor behavior in the work situation.

*Situational factors in leadership.*—Although some studies [Mann & Hoffman (67); Pepinsky, Hemphill & Shevitz (82); Solem (102); and Stanton (104)] have been concerned with the effects of situational variables such as company policies and leadership climate on leadership, most investigations have treated situational properties as conditioning variables, i.e., variables which interact with leader traits or behaviors in the determination of leadership. The effectiveness of specific leader characteristics is hypothesized to depend on certain properties of the situation in which they are expressed.

Likert has recently reviewed the earlier research based on this hypothesis and concludes,

Supervision is, therefore, always an adaptive process. A leader to be effective must always adapt his behavior to fit the expectations, values and interpersonal skills of those with whom he is interacting (53, p. 327).

Mann & Hoffman (67) compared relationships of both management identification and foremen's power with subordinates' satisfaction in an automated and in a less automated power plant. Men in the automated plant

were found to be more satisfied with foremen having high power; no difference was found in the less automated plant. The authors interpret this finding in terms of additional evidence which indicates that, generally, the foremen in the automated plant had a higher level of human-relations skill. Power on the part of the foremen is hypothesized to contribute to subordinate satisfaction only when the foreman is seen as considerate of the interests of his men. Similar evidence concerning the interaction between power and human-relations skill was obtained previously by Pelz (81).

Much of the recent research on democratic and participative supervision is also based on a situational point of view. The question is no longer, does democratic leadership work, but rather, under what conditions does it work? Some empirical evidence concerning conditions which determine the effectiveness of democratic processes has been obtained in a number of recent studies. French, Israel & As found that the positive effects of participation on workers' attitudes was directly related to the legitimacy of participation. Legitimacy was defined as "the extent to which the parties involved consider it right and proper to engage in the decision-making process" (23, p. 5). Similarly, support was obtained for their hypothesis that the effects of participation increase with decreasing resistance to the participation methods.

Vroom (115, 117) demonstrated that the consequences of psychological participation, defined as the amount of influence an individual feels he has in decision making, varied with certain personality characteristics of the participant. Intercorrelations between amount of participation and measures of job satisfaction and job performance were computed for the entire sample of 108 supervisors and for subgroups representing different degrees of authoritarianism and need for independence. Although the over-all relationship between participation and both satisfaction and productivity was positive, the magnitude of this relationship was found to vary with degree of authoritarianism (as measured by the F-scale) and strength of need for independence of the participant. The evidence suggests that authoritarians and persons with weak independence needs are unaffected by participation. On the other hand, equalitarians (persons with low F-scale scores) and persons with strong needs for independence become more satisfied and perform better on their jobs as a result of participation in decision making. These results are similar to those of Zipf (127) who found that individuals with strong independence needs showed less conformity to repeated influence attempts based on both reward and coercive power.

There is some evidence that the consequences of leader characteristics vary with certain structural properties of the group. Vroom & Mann (118) found substantial differences between effects of leader authoritarianism on the attitudes of employees in two kinds of work groups within a single industrial organization. Employees in small, highly interdependent work groups which were characterized by a great deal of interaction among

workers and between workers and their supervisor had more positive attitudes toward equalitarian leaders. The correlations between the supervisor's F-scale score and the mean attitude toward the supervisor of his subordinate was  $-.41$  for 24 groups. In contrast, employees in large work groups in which opportunities for interaction among workers and between workers and their supervisor were greatly restricted and in which individual employees were highly independent were found to have more positive attitudes toward authoritarian leaders. The correlation between the F-scale scores and mean subordinate attitudes toward the supervisor was  $+.41$  for 28 groups.

The theoretical position of Newcomb (79) concerning the interdependence of interpersonal similarity and attraction has influenced recent research on the supervisory-subordinate relationship. A number of studies have been carried out to determine the relationship between the amount of similarity or agreement between supervisor and subordinate and their attraction toward or evaluation of each other. Triandis (110, 111) used two types of measures of similarity of supervisor and subordinate—categoric similarity based on their responses to an adaptation of Kelly's Role Repertory Test, and syndetic similarity based on Osgood's semantic differential. Two kinds of objects, persons and jobs, were used with each method of measuring similarity. He found a significant relationship between categoric similarity of supervisor and subordinate based on people and syndetic similarity based on jobs and measures of both the effectiveness of interpersonal communication and liking within the pair.

Meyer (75) and Pfefferkorn [cited in Lawshe (46), p. 291] have attempted to determine the relationship between the amount of agreement between supervisor and subordinate with respect to the subordinate's role and the evaluation of the subordinate by the superior. Meyer found no relationship between measures of these two variables, whereas Pfefferkorn found a positive relationship.

These results suggest the inadequacy of broad generalizations concerning the effects of leadership styles. An adequate theoretical explanation should include a consideration of personality characteristics of subordinates and certain social structural characteristics of the work situation.

*Leadership development.*—Likert (56) expresses dissatisfaction with current methods of reviewing or appraising performance in industry and suggests replacing the subjective judgment of the superior with objective measurements of organizational performance. He outlines an approach to management development in which each manager evaluates his own performance on the basis of periodic measurements of production costs and waste, and of such human-relations variables as communication and group loyalty. According to this program each manager works with his own subordinates in setting objectives for the period ahead and reviews these objectives with his superior. At the end of that period the degree to which

the objectives have been attained is assessed and studied by the manager and his subordinates who then set new objectives and plans for the next period.

Some evidence supporting Likert's contention that objective feedback improves group performance was obtained in a laboratory experiment carried out by Pryer & Bass (83). Groups receiving feedback made significantly more accurate group decisions concerning the relative population of cities. Since the nature of the task ruled out learning of content, it was inferred that feedback affected group performance either by enabling group members to learn how to tackle problems effectively or by stimulating group motivation or interest in the task.

Maier (59) is also skeptical about the utility of the usual form of appraisal interview for purposes of executive development, but sees the solution as lying in the methods used in the appraisal interview. He distinguishes three methods of appraisal interviewing: (a) "the tell-and-sell" method in which the supervisor communicates his evaluation to the subordinate and then attempts to "sell" him on how to improve, (b) "the tell-and-listen method" in which the supervisor communicates his evaluation to the subordinate and then attempts to explore the subordinate's feelings about the evaluation, and (c) "the problem-solving method" in which there is no communication of evaluation to the subordinate—the focus of the interview is on the problems encountered in the work situation. Maier feels that the problem-solving method, in which the superior acts as a helper rather than a judge, offers the greatest promise for exploring obstacles to the subordinate's growth and development and for attaining high-quality and mutually acceptable solutions to these obstacles. Since performance and job problems are closely linked, a discussion of the job leads to ideas for improving performance.

Continuing interest on the part of psychologists in evaluating human-relations training courses is indicated by a number of recent studies. The findings indicate that training changes leaders' conceptions of how they should behave [Spector (103)], their ability to analyze causes of employee behavior in a training film [Lawshe, Bolda & Brune (47)], and their actual behavior in classroom role playing [Maier, Hoffman & Lansky (63); Mann & Mann (70)]. Although there appears to be little doubt that training does produce changes in trainees, there is no evidence from these studies concerning the degree to which these changes are reflected in job behavior or group effectiveness. Triandis' (109) finding that supervisors who took a work simplification course turned in a substantial number of methods improvements resulting in considerable financial saving suffers greatly from the lack of a control group and information concerning the number of pre-training improvements suggested by the trained group.

In an article describing the evaluation of the Bell Telephone Program of Humanistic Studies for Executives, Viteles (113) raises some questions

about the value of human-relations training. Courses in the program, which lasts for a 10-month period, are grouped into four major fields—history, science, philosophy, and the arts. Before-after tests given to students in the first three years of the program and to a control group reveal that the course has a significant impact on both the knowledge and values of participants. The findings demonstrate that the course produces consistent changes in the direction of more liberal attitudes, and results in an increase in the strength of artistic and aesthetic values and a decrease in the strength of economic values. Although these findings are supportive of this bold experiment in management development, it is too soon, as Viteles points out, to determine how long-lasting these attitudinal changes will be or the extent to which they will result in behavioral changes both on and off the job.

A positive development in a number of recent training evaluation studies is the attempt to determine the effects of specific training methods rather than to evaluate an entire training program in which a combination of methods has been used. Lawshe, Bolda & Brune (47) and Mann & Mann (70) have investigated the effects of role playing; Smith & Kight (101) evaluated the effects of feedback within the training situation. The objective of the latter study was to determine the effects of feedback within training groups on the self-insight of supervisors and on their problem-solving effectiveness in small groups. Supervisors whose training experience included daily interaction in small three- or four-person groups, organized for the purpose of providing them with feedback concerning the effects of their behavior on other group members, had greater self-insight than those whose training did not include feedback. Similarly, subgroups in which feedback discussions were carried out demonstrated greater effectiveness in group problem solving than subgroups organized for different purposes or control groups in which there had been no previous subgroup experience during the training course. These findings provide some support for the emphasis placed on such feedback in "sensitivity training" described in recent books by Miles (76) and by Weschler & Reisel (121).

Few efforts to deal with the process of leadership development have been concerned with individual differences in response to training methods. Studies by McClintock (73) and Goldstein (25) suggest that the effects of a given attitude-change procedure vary with certain personality characteristics on the part of the person. It seems likely that the effectiveness of lectures, group discussion, role playing, etc., as development methods may depend on the motivational basis of the particular attitude or behavior to be changed and might vary greatly from person to person and from one subject matter to another. Further research on the development process might be directed toward interactions between situational and personality variables.

This emphasis might profitably be accompanied by a greater attempt to



conceptualize training variables in psychologically meaningful terms. As people like McGehee (74) have pointed out, there is a need for a greater interchange between theorists interested in human learning and attitude change and persons concerned with the design, execution, and evaluation of training procedures.

Lippitt, Watson & Westley (57) attempt to provide a general framework for the analysis of "planned change," defined as a change that originates in a decision to make a deliberate effort to improve the system and to obtain the help of an outside agent in making this improvement. The authors are not concerned exclusively with leadership development but with the broader question of the contribution of external agents to improving the functioning of four types of dynamic systems—the individual personality, the face-to-face group, the organization, and the community. The objectives and methods-of-change agents in various professions are compared, and some fairly universal phases in the change procedure are identified. This volume serves to integrate the methods and theories of hitherto unrelated disciplines and represents an important contribution both to the theory and to the technology of planned change.

#### ORGANIZATIONAL PROBLEM SOLVING AND DECISION MAKING

Most of the research on joint problem solving and decision making has been conducted in relatively unstructured laboratory situations. The data on decision making in ongoing organizations, reviewed by Lazarsfeld (48) and contained in an annotated bibliography by Wasserman & Silander (120), are fragmentary and have been obtained largely from case studies.

March & Simon (72) have recently emphasized the importance of a concept of rational man in organizations. Over one-third of their new volume is devoted to a discussion of the decision-making and problem-solving aspects of organizational behavior. They point out that the study of organizations was dominated in the first quarter of this century by the advocates of scientific management who viewed human actors as "instruments" describable in terms of a few physiological and psychological properties. During the second quarter of the century the human-relations point of view took precedence, and attention was focused on the needs and feelings of organization members. Neither of these approaches was concerned directly with the cognitive and rational properties of the individual.

Cyert & March (15) make an important distinction between two lines of theoretical development in the area of organizational decision making. Normative theorists are concerned with improving the rationality of organizational choice and attempt to specify how organizational decisions should be made. Empirical theorists, on the other hand, are interested in understanding how complex organizations actually do make decisions. Despite the fact that recent developments in operations research have contributed to our knowledge of how organizational decisions should be made

[Dean (16)], Cyert & March feel that we still know comparatively little about how these decisions are actually made. They caution that organizational decision making should not be viewed in the same terms as individual decision making. Choice by organizations is a legitimate focus of research in its own right and may be mediated by quite different processes than choice by individuals.

The inadequacy of traditional economic theory for dealing with organizational decision making has been emphasized by a number of writers. The classical economic man makes a choice from among a well-defined set of alternatives for which the consequences are well known, and his decisions are made so that the organizational value of the outcome is maximal. Although these postulates may describe how a choice ought to occur, they do not represent the typical decision-making situation in industry. The alternatives are not always well defined and depend on the amount of search undertaken (14). Decision makers often have incomplete knowledge concerning the consequences of each of a number of possible actions (14, 72, 97). There is no single criterion in terms of which alternatives can be evaluated (14). Finally, decisions often reflect the hopes or emotional preferences of the decision makers (14).

The weaknesses of classical economic decision-making theory are widely recognized; however, the paucity of our empirical knowledge and the complexity of the problem would seem to make an attempt to formulate a substitute theory premature. Consequently, the current emphasis on the definition of concepts and on the formulation of problems appears to be entirely appropriate.

Maier (59, 60) distinguishes between problem solving and decision making. Decision making implies a number of given alternatives, whereas in problem solving the alternatives must be created. Thus, problem solving has two major aspects—finding or creating alternatives and evaluation and choice between alternatives. He emphasizes that they are not only conceptually separable, but should, in fact, be separated temporally. At the outset of a discussion it is important to present a number of possibilities for consideration without passing evaluative opinions or judgments on any one of them. The judging or evaluation of ideas during this stage will inhibit creativity and freedom of thought. If it is assumed that groups asked to solve a problem for the second time are more likely to explore a number of solutions and are relatively free of the need to make rapid evaluations, Maier's ideas are consistent with the finding that groups attained higher quality second solutions to problems which they had previously solved (61).

Maier (59) discusses the leadership skills conducive to effective group problem solving. Fundamental to his position is the notion that joint problem solving by supervisors and subordinates can lead potentially to decisions of greater quality and acceptance. According to Maier, one of the most important skills is the statement of the problem. Problems stated in

(a) situational rather than behavioral terms, (b) a form of interest both to the superior and the subordinate, and (c) such a way as not to imply a solution are hypothesized to be most successful in generating creative and inventive problem solutions. He also views the role of the superior in the discussion as playing a large part in determining the outcome, once the problem is stated. Frequently, poor solutions to problems are accepted because there is a tendency to adopt the first solution that is found. Skills and attitudes conducive to the idea-getting stage of problem solving include sensitivity to feelings, receptivity to ideas, an acceptant and patient manner, and the ability to restate an idea concisely.

Taylor, Berry & Block (107) carried out an experiment comparing the effectiveness of group and individual methods of generating problem solutions. Twelve four-man groups and 48 individuals followed the same rules of brainstorming in attacking these problems. The 48 individuals were then divided into nominal groups of four men each on the basis of a table of random numbers. The performance of the real groups was found to be markedly inferior with respect to (a) the mean total number of ideas produced, (b) the mean number of unique ideas produced, and (c) three different measures of solution quality. These results, which are similar to those of Faust (19), cast some doubt on the advantage of group participation in the aspect of the decision-making process which involves the generation of alternative solutions to problems. Both studies, however, must be reconciled with Barnlund's evidence (3) that group decisions reached through cooperative deliberation on a task involving ability to draw logical conclusions from given arguments were significantly superior to decisions made by individuals working alone and to majority rule decisions obtained by pooling individual answers of group members.

It seems likely that the relative performance of individuals and groups depends on a number of other variables. Group interaction may be of less importance in generating alternative solutions to problems than in evaluating and selecting among alternatives. Furthermore, there may be substantial differences between the kinds of problems for which interaction is facilitative as opposed to being a source of interference. The question of the relative superiority of groups and individuals in problem solving might be fruitfully rephrased to take into consideration such additional factors as the nature of tasks, leadership, and interests and abilities of group members. Roby & Lanzetta (90) have described an approach to the conceptualization of the differences between tasks used in problem-solving studies which may prove helpful in this regard.

The second aspect of the decision-making process involves the comparison and evaluation of proposed alternatives and consequent selection among them. Evaluation of alternatives inevitably involves the notion of organizational goals or objectives. Alternatives are typically compared in terms of the extent to which they are expected to contribute to one or more desired end states.

As Cyert & March (15) have pointed out, however, not all goals are likely to be considered at the same time. A multiple fatality, for example, is likely to direct attention to a safety goal and increase the probability that decisions will reflect safety considerations. Subsequently, events may focus attention on public-relations goals, and safety will once again be cast aside. They propose the concept of attention focus to describe fluctuations in the saliency of various organizational goals.

Cyert, Dill & March (14) examine four case histories of business decision making and suggest that there are several stages to the comparison and evaluation process. Initially, only rough expectation data are used to screen obviously inappropriate actions. As the decision approaches implementation, the search for evaluative information becomes more and more intensive. Decisions are typically made within existing budgetary constraints and, particularly under conditions of abundant organizational resources, there is only the most obvious comparison of solutions to different problems. The evaluation procedure is generally restricted to the comparison of expected costs and returns of alternative solutions to the same problem, with little attempt to determine whether expected net return on this investment equals or exceeds the expected return on all alternative investments.

Although, in general, the comparison of alternatives in terms of their contribution to a solution of a particular problem rather than organizational objectives simplifies the decision-making process greatly, the problem of estimating the relative value of alternative actions is a difficult one. Information on dollar costs and dollar savings is not easy to obtain and often unreliable. Furthermore, not all of the relevant consequences can be reduced to an economic criterion. It is impossible to estimate the dollar value of consequences like safety of personnel and reputation of the company. In the absence of complete knowledge of the consequences of alternatives or even definite probabilities of these consequences, motivational factors often influence the result. In each of the cases studied by Cyert, Dill & March there is evidence that expectations are influenced by the conscious or unconscious motives of the decision maker.

Working within a normative framework, Maier (60) proposes four impersonal screening principles in terms of which a group can eliminate poor-quality solutions and select those with high quality. Two of the principles specify alternatives which should be rejected: (a) solutions transferred from other problems and (b) solutions supported by facts or interpretations of facts that are challenged by other members of a group. The other two principles concern solutions which should be selected for further consideration: (a) those solutions founded either upon any of the unchallenged facts or upon unchallenged interpretations of facts taken from the problem situation and (b) those solutions based upon a trend and results, the exceptions to which can be satisfactorily explained. Solutions selected by the positive principles are evaluated by group discussion

in terms of: cost and practical considerations, the way in which they may be integrated, a selection from among alternatives when two actions are incompatible, an examination of the extent of support given by facts or interpretations, and acceptability to group members.

A number of laboratory experiments have been concerned with the relationship between various aspects of group structure and the quality of decisions and their acceptance by group members. Solem (102) found that full delegation of authority by the leader to group members produced higher-quality decisions and greater satisfaction with decisions both by the leader and subordinates. Hoffman (39) found that nonhomogeneous groups in which members had dissimilar personality profiles on the Guilford Zimmerman Temperament Survey produced higher-quality solutions than homogeneous groups in which members had similar personality profiles. No significant differences were found in degree of satisfaction with solutions or in the attractiveness of group members to one another over an extended period.

Following up the Bavelas (7) and Leavitt (50) findings concerning the effects of communication structures on group problem solving, Mulder (78) found that in the beginning of the work period "wheel" groups made more errors and required more time per problem than "circle" groups. Later on in the work period, however, the relation was reversed with wheel groups exhibiting performance superior to circle groups. He also found that groups of both wheel and circular types which developed centralized decision structures demonstrated greater speed, quality, and efficiency in problem solving than those with less centralized decision structures. The reversal in relationship over time between communication structure and performance is attributed to the greater "vulnerability" of wheel-type groups when a centralized decision structure has not yet developed.

Kennedy (44) and Hoggatt (41) have recently described the relevance of business games to the study of organizational decision making. Although ordinarily used for management training purposes, these experimental games seem to offer considerable promise as research tools. The difficulties of observation and experimental control involved in the study of decision making in on-going social organizations may be overcome by simulating such situations in the research laboratory.

#### THE MOTIVATIONAL CONSEQUENCES OF TASK VARIABLES

The motivational effect of the nature of the tasks performed by the individual continues to be a relatively neglected problem in psychology. Engineering psychologists have contributed much to the design of tasks consistent with the sensory, motor, and cognitive capacities of individuals; however, comparatively little is known about the effect of job content on the motivation and satisfaction of persons performing them. Psychologists have stressed the importance of supervision and interpersonal relations and

have devoted comparatively little effort to the conceptualization of the tasks performed by individuals in motivationally relevant terms.

The importance of task variables to employee satisfaction and motivation is illustrated in a recent study carried out by Herzberg, Mausner & Snyderman (38). Accountants and engineers were asked to describe times during which they felt exceptionally good and exceptionally bad about their jobs. Most of the stories about favorable periods emphasized the actual job carried out by the person. The factors most frequently mentioned as sources of satisfaction were achievement, recognition, the work itself, responsibility, and advancement. On the other hand, stories told by respondents concerning periods in which they felt negatively about their jobs most frequently involved the job context, i.e., company policy and administration and supervision. The authors conclude from these findings that factors associated with the job itself typically bring about job satisfaction, but their absence does not result in dissatisfaction. Moreover, the job context can produce dissatisfaction but not satisfaction.

Similar findings have been obtained by Gurin, Veroff & Feld (28) in an interview study of a national sample of employed men. They found that ego-factors (including the work itself and the opportunities it affords for the use of skills) are mentioned much more often as positive characteristics of the job, whereas extrinsic factors (including money, job security, and working conditions) are more frequently noted as complaints than as sources of satisfaction.

The legitimacy of the Herzberg, Mausner & Snyderman conclusion that there is a qualitative difference between objective conditions acting as satisfiers and those acting as dissatisfiers is open to question. There is a risk in inferring the actual causes of satisfaction and dissatisfaction from descriptions of events by individuals. It seems possible that the obtained differences between events may reflect defensive processes at work within the individual. Individuals may be more likely to perceive the causes of satisfaction within the self and hence describe experiences involving their own achievement, recognition, or advancement in their job. On the other hand, they may tend to attribute dissatisfaction not to personal inadequacies or deficiencies but to factors in the work environment, i.e., obstacles presented by company policies and supervision. This type of explanation is invoked by Gurin, Veroff & Feld, who, while noting the possibility of the Herzberg *et al.* interpretation, state that

... this finding suggests that there is relatively little introspection in analyzing the sources of distress on the job—that complaints tend to be externalized rather than cast in personality terms (28, p. 153).

Argyris (2) compared the interview responses of employees in two departments—one made up of jobs requiring a high degree of skill and the other primarily unskilled or semiskilled. He found that employees in the



higher skilled department expressed greater interest in their jobs, greater sense of self-worth related to their technical competence, and greater appreciation for high-quality work, and placed less emphasis on money. Cost-control records also showed less spoilage of work in the department with the highly skilled jobs. The statistical significance of these differences is impressive, but it is impossible to determine from the data the extent to which they reflect motivational effects of tasks as opposed to differences in the kinds of persons who select or are selected for the two kinds of jobs. Furthermore, the methodology employed makes it extremely difficult, if not impossible, to separate the effects of the skill level of the jobs from other factors distinguishing the two departments.

Kennedy & O'Neill (43) cast some doubt on the assumption that the variety in tasks within a job markedly affects employee attitudes. They compared the attitudes of assembly operators each of whom performs a single routine task with those of utility assembly operators who perform a wide variety of these routine tasks. No differences were observed. These findings have implications for advocates of job enlargement or job rotation. Greater variety of tasks may not increase satisfaction unless the tasks form a unified, integrated, and meaningful whole. Enlarging the job by adding diverse, unrelated activities or rotating the worker from one job to another unrelated job may not have the intended positive consequences on either satisfaction or motivation.

Slater (100) has studied the effects of some job characteristics on the internalization of motivation toward occupational role performance among blue-collar workers in an oil refinery. Motivation was defined as internalized to the extent that it is independent of externally mediated sanctions. The amount of internalization of occupational role performance on the part of an employee was found to be positively associated with management's ratings of the amount of aptitude required in his job and the employee's own perception of the amount of self-determination on the job.

The relationships between occupation and criteria of mental health and adjustment have been studied by a number of researchers. In the study described earlier, Gurin, Veroff & Feld found a positive relationship between occupational status and both job satisfaction and feelings of adequacy on the job. However, the lower-middle class white-collar occupational categories of "clerical" and "sales" reported lower scores on mental health criteria than would be predicted from their occupational status alone. About 10 per cent more of the men in these groups than in any other occupational category except the unskilled group expressed dissatisfaction, and a larger percentage in these groups than in any other category report that they have experienced problems on the job. Furthermore, 57 per cent of the clerical group expressed a desire for some other kind of work, a figure at least 10 per cent higher than that obtained in any other category. The authors suggest that white-collar occupations "maximize the frustration that derives from the nonfulfillment of high ego involvement and aspiration" (28, p. 162).

Lee & Schneider (51) failed to find evidence for the popular notion that the stresses on top executives frequently lead to hypertension and arteriosclerosis. On the basis of extensive medical observation of 1171 male executives and 1203 nonexecutives the authors report no evidence for greater incidence of either of these two disorders in executives. On the contrary, there was a tendency, which approached significance, for nonexecutives to have more hypertension.

A number of recent studies have been concerned with the job changes associated with automation and the resultant effects of these changes on the satisfaction and motivation of workers. Walker reports some of the results of his 6½ years of observation in a semiautomatic steel factory. The introduction of automatic technology greatly reduced the amount of physical effort required by jobs but placed significantly more mental demands on the worker. Jobs in the semiautomatic steel plant also demand more attention from the workers and give them greater responsibility. They are required "to watch all the time, to act on split-second notice, and never to make mistakes, because of the dire consequences to machine and product, and even to themselves" (119, p. 16). In describing the attitudinal and behavioral effects of these job changes, Walker suggests that the immediate consequences are negative, but that the negative reactions are gradually replaced with positive ones. Workers typically complain of the mental fatigue and the risks inherent in their new tasks. In time, however, much of the resentment disappears, and those aspects of the job which were disliked become prime sources of satisfaction.

Some of Walker's observations are supported by a quantitative study of an automated and a nonautomated power plant carried out by Mann & Hoffman (67). Technological changes in the automated power plant resulted in enlarging the jobs of each operator, increasing the scope of responsibility on each of the jobs, and rotating workers among different types of jobs. Operators transferred from the less automated to the automated plant reported that they had more responsibility on their jobs and that their new jobs required much more training. The over-all effects of the change in job content were positive, and transferred workers expressed greater satisfaction with their new work. Some negative effects in the form of increased tension were observed, but are attributed by the authors largely to inadequacy of preparation for the transition from old to new jobs. The absence of the dissatisfaction reported by Walker may reflect the fact that at the time the attitudinal measurements were taken, most of the operators had been on their new jobs for about a year and a half.

Mann & Williams (68, 69) report a case study of automation in a white-collar setting. Over a period of years they observed the consequences of change-over to electronic data-processing equipment in the accounting department of a large public utility. In addition to the elimination of highly routine and menial jobs, the introduction of the complex computer system also reduced the number of high-level nonsupervisory jobs. The intermedi-

ate level positions, previously highly specialized, underwent considerable enlargement which meant that each person was trained to handle the operations formerly involved in five different jobs. These new jobs also gave more responsibility to the individual employee. Errors were more costly and more easily attributed to the person who made them.

Levinson (52) and Gross, Mason & McEachern (27) have expressed dissatisfaction with the use of the role concept in studies of organizations and suggest reformulations of the concept. The latter have carried out an extremely rigorous and systematic study of the school superintendent's role with particular attention to the determinants and consequences of consensus in role expectations. Among the extremely provocative results of this study is the finding that satisfaction of school board members was positively related to the amount of role consensus among them (intraposition consensus). There was, however, no relationship between the satisfaction of the superintendent and his degree of agreement with the board (interposition consensus). The authors conclude that satisfaction and consensus are related within an interacting group of incumbents of the same position but not between a group and an incumbent of a counter position.

It may well be, however, that the crucial variable affecting the relationship between consensus and satisfaction is not amount of interaction between persons, or whether or not they are occupants of the same or different positions, but the amount of role interdependence among them. If the roles of two persons are interdependent, i.e., neither could perform his role without assistance or clearance from the other, it seems likely that lack of agreement on respective role definitions would represent a constant source of frustration. This might be the case with a school board which cannot function without a certain amount of agreement between members. On the other hand, as Gross *et al.* point out, the superintendent and board can continue to function even though they disagree with one another completely. These findings suggest that the explanations of motivational consequences of the tasks or functions performed by a worker need to focus not only on the nature of the tasks and functions themselves, but also on the amount of consensus with respect to these tasks and functions between the worker and those performing roles which are interdependent with his own.

#### ORGANIZATIONAL STRUCTURE

*Small group organization.*—Stemming from the early work of Elton Mayo and the Hawthorne studies, attention continues to be directed toward the primary work group as a potential source of worker satisfaction and productivity. Likert has described a motivational approach to a modified theory of management in which the face-to-face work group plays a primary role. He states,

management will make full use of the potential capacities of its human resources only when each person in an organization is a member of one or more well-knit,

effectively functioning work groups that have high skills of interaction and high performance goals (55, p. 192).

These work teams are linked together into an over-all organization by means of supervisors who are members of more than one work group. The Likert model calls for replacing the man-to-man pattern of supervision required by traditional management theory with interaction between supervisors and all subordinates. Such interaction would be characterized by effective communication and influence and decentralized decision making and, in addition, would provide an atmosphere in which each person would derive a sense of personal worth. Likert's model is similar to that of the interlocking primary groups described by Fraser (22) in his recent book, and not unlike the communication diagram described by Maier (58) in connection with the group-decision process.

Some empirical evidence concerning the determinants and consequences of membership in a primary work group was obtained in a study carried out by Zalesnik, Christensen & Roethlisberger. Although the number of cases studied is rather small, 47 persons in four work groups, the findings provide some support for the authors' assumption that "productivity is an expression of conformity to or deviation from a norm of behavior which informally prescribes what the group expects of its members" (126, p. 221). The authors deal with three classes of group members—regulars, deviants, and isolates—although the conceptual and operational bases for this distinction are not made clear. Regular group members are typically high in status and in status congruence (i.e., the degree to which they occupy the same position on different status factors). Persons high in status and low in status congruence were most frequently isolates, whereas persons low in status, regardless of degree of status congruence, were deviants. Regular group members tended to be highly satisfied and to abide by group norms of production. Isolates and deviants, on the other hand, tended to be dissatisfied and to deviate from group norms of production.

Sayles (92) conducted an exploratory study to determine the consequences of technology and organization of work on work-group behavior. He describes four types of groups on the basis of their behavior with respect to the grievance system—apathetic, erratic, strategic, and conservative. These and other aspects of group behavior were found to be associated with the amount of homogeneity and degree of interdependence of tasks performed by different persons in the work group. While the sample was very impressive (300 groups in 30 plants in a variety of industries), the usefulness of the study is greatly restricted by the methodology employed. The data, obtained by unstructured interviews with management and union, were not quantified, and no statistical analyses were performed. Though extremely provocative, the findings are, perhaps, more appropriately treated as hypotheses for further research.

Following up on the Trist & Bamforth (112) studies on the longwall

method of coal mining, Rice (89) carried out a series of field experiments in a large textile mill in India. Modern machinery and modern working methods had been introduced prior to the study but had not proven as successful as had been expected. Rice introduced a number of changes in work organization including the formation of small, internally structured work groups. Subsequent increases both in quantity and quality of production were attributed to the fact that the new work organization satisfied the affiliative needs of workers. The author feels that, in most societies, the introduction of technological changes and the breakdown of traditional institutions have resulted in some degree of social isolation which should be compensated for by experiences within the work situation. Consequently, it is important to organize tasks in such a way as to enable workers to derive social and psychological satisfactions from their membership in primary work groups. The effects of this action research program on organizational effectiveness are impressive; however, the complex nature of the changes introduced and the absence of a control group, or of measures of attitudes, or perceptions of workers makes it difficult to draw unequivocal conclusions concerning the underlying processes.

The efforts of Rice and of Sayles to explain group behavior in terms of the relationships between the work roles of group members may be contrasted with that of Schutz (94), who describes a theoretical and methodological approach to the problem of predicting group effectiveness from the compatibility of personalities of group members. Although not concerned specifically with industrial work groups, many of Schutz' hypotheses are extremely relevant to the industrial situation.

From a study of 125 workers transferred from a nonautomated to an automated automobile plant, Faunce concludes,

Changes in informal social structure resulting from automation point to the necessity for a more rigorous definition of the work group concept and to the need for caution in the application of the concept in analysis of the consequences of group activity (18, p. 407).

He found that workers in the automated factory had less opportunity to interact with others because of the somewhat greater attention required by the jobs and the greater distance between work stations. In general, there were more feelings of social isolation, less teamwork, and less interdependence than in the nonautomated plant. The generality of these findings as necessary concomitants of automation may be questioned in the light of the Mann & Williams conclusion that greater interdependence among workers resulted from the introduction of automatic data-processing equipment (68) and Walker's statement that automation had no effects on group cohesiveness in a steel plant (119).

*Large scale organization.*—Although there have been many efforts to conceptualize and theorize concerning the macro-structural aspects of or-

ganizations in recent years, Haire's observations (32, 33) on the paucity of empirical data in this field seem entirely justified. There are many difficulties in collecting data from a sufficient number of large-scale organizations to permit adequate tests of existing theories. Many of the existing research investigations are case studies of a single organization or comparisons between two or three organizations, making it extremely difficult to draw conclusions about causal relations between variables.

Revans reviewed the evidence concerning the relationship between organizational size and organizational functioning. On the basis of generally positive relationships between size and absences, accidents, lateness, and strikes and a curvilinear relationship between size and job performance, he concludes that "some ostensible aims of human beings are difficult to achieve in large organizations" (88, p. 182).

Baumgartel & Sobel (6) found a strong positive relationship between size of plant and absence rates in 11 organizational units of an air line varying in size from 172 to 3205 employees. The relationship between size and absences was found to hold not only for the total population, but also for each of 12 subpopulations formed by dichotomizing workers on the basis of each of six control variables. These findings are explained in terms of an assumed association of organizational size with impersonality, lack of personal freedom, and diffuse identifications with the organization, all of which tend to reduce the attractiveness of the organization to the person. No data were obtained on any of these intervening variables nor on the average size of work groups within plants. Consequently, it is difficult to relate this study to others showing a positive relationship between size of work group and absences.

Thomas (108) studied the role conceptions and quality of performance of 109 welfare workers in small, medium, and large organizational units of a state welfare department. The smaller units were characterized by more role consensus, greater breadth of role conception, higher ethical commitment, and higher quality of work performance. In interpreting the results, however, the author minimizes the part played by size alone and suggests that the findings reflect differences in community settings associated with bureaus of different size.

Haire (30) has likened growth of organizations to that of biological organisms. He feels that the principles of development of living organisms can provide a basis for the derivation of hypotheses concerning the interdependence of size, shape, and function in organizations. Evidence for this similarity is obtained through an analysis of the growth records of four industrial firms. For example, the square cube law—as volume increases by a cubic function, the surface enclosing it increases only by a square—might be applied not only to physical bodies, but also to organizations. The volume of the firm was expressed in terms of the number of "inside" employees (i.e., persons primarily concerned with functions inside the firm) while its sur-



face was represented by the number of "outside" employees (e.g., purchasing, shipping, receptionists, etc.). Graphically, the relationship between the square root of the number of external employees and the cube root of the number of internal employees for each of the four firms approximated a straight line in each case. Data were also collected on internal changes in the organization associated with growth, including the ratios of staff to line and supervisors to subordinates and the percentages of clerical and top and middle management in the organization at various stages in its growth. Although only four firms have been studied up to this time, some interesting patterns and regularities are emerging which merit further attention. The phenomena which Haire is studying seem novel and intriguing, but the usefulness of the biological analogy is less clear.

Cartwright has described the potential applicability of graph theory to a theory of organizations. Units of an organization, e.g., persons or roles, are represented by points on a graph, and relationships between units, e.g., authority, communication, interdependence, are represented by lines on the graph. Apart from the advantage of depicting structural organizational properties, Cartwright sees the mathematical properties of graph theory as having three potential contributions to organization theory. First, graph theory can contribute to the "language" of organization theory through its higher order concepts, which are rigorously defined in terms of a mathematical system. Second, graph theory contributes valuable techniques of computation and formulas for calculating quantitative features of organization structure. Finally, "the theorems of graph theory specify features of graphs, and thus of organizational structure, which follow necessarily from the undefined terms and axioms of graph theory" (11, p. 256).

Harary (34) shows how certain organizational concepts such as liaison persons and cliques can be defined rigorously in terms of digraph theory. He introduces some new concepts, such as strengthening and weakening group members and status and contrastatus, which, although suggested by the mathematical system, would seem to have important empirical consequences. A strengthening member of a group is one whose presence results in a more strongly connected group. Ross & Harary (91) derive the somewhat startling, but provable, theorem that any group has at most two weakening members. Status is defined positionally and depends not only on the number of subordinates, but also on their distribution into levels. The contrastatus of a person is his status in the inverted organization chart (35).

The method for the measurement of influence structure introduced by Tannenbaum & Georgopoulos (105) and applied by them in a study of two industrial plants has also been used in studies of four union locals [Tannenbaum & Kahn (106)] and a staff division of a large company [Williams, Hoffman & Mann (124)]. There are some conceptual and methodological problems to be worked out in connection with the method (124); however, it represents one of the few attempts to measure objectively important

structures in on-going organizations. The data collected up to this time have suggested some hypotheses about the determinants and consequences of influence structure. Further research involving a larger number of organizations is needed in order to test these hypotheses adequately.

#### CONCLUSION

The field of industrial social psychology shows definite signs of progress during the period covered by this review. Together with their colleagues in other social sciences, more psychologists are discovering that organizations represent highly fertile ground for the development and testing of theories and hypotheses. As a result, a larger number of organizational phenomena are being subjected to scientific study and analysis. Old problems, such as the relationship of morale to productivity, are being expressed in more theoretical terms, and new problems in such areas as communication and decision making in organizations are emerging and being tackled with vigor and imagination.

Industrial problems still exert some influence on the phenomena studied, but the nature of research appears to be affected more by theoretical considerations and somewhat less by demands on the part of organizations for immediate practical results. This theoretical emphasis is permitting a greater rapprochement between the theories, methods, and findings developed in the laboratory and those generated in field situations. Although "basic" research in industrial organizations may seem to leave practical organization problems far behind, it should ultimately provide a much sounder basis for administrative action.

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## CONCEPT FORMATION<sup>1,2,3</sup>

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Definitions that are intended to delineate areas of scientific explanation inevitably get fuzzy where they are most needed—at the boundaries. Nevertheless, some rough framework can serve a useful purpose if it does not serve to constrict the researcher or become a basis for argument. In this spirit, the first annual review of concept formation offers the definition that helped to sort the literature and the issues covered. Concept formation is taken to imply the acquisition or utilization, or both, of a common response to dissimilar stimuli.<sup>4</sup> It is the problem of those who study concept formation to analyze the process and determine which variables influence it.

The early scientific approach attempted to analyze concept formation by studying the conscious experience attendant upon it. Watt (132), for example, working in the Würzburg laboratory, asked his "observers" to report on their conscious processes when they were given tasks such as naming a superordinate for a subordinate, or a part for a whole. He discovered that if the observer is adequately prepared, there is little or no observable conscious content. The question is put and the answer comes automatically. Thus, the stage was set for a behaviorist approach and Ach (1) helped raise the curtain. Boring (7) reports that, with Ach

... it became clear that the problems of thought and action are essentially the same. In both cases one has some specific end to achieve, and the psychophysical process, released by a stimulus, runs its course to that end. To name a rime for a stimulus-word is psychologically no different from pressing a given finger when a given letter appears.

Nevertheless, Ach and his contemporaries continued to experiment within

<sup>1</sup> In the survey of the literature pertaining to this review, concentration has been on the period from April, 1957 to April, 1960.

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<sup>3</sup> The following abbreviations are used: S-R (stimulus-response); CS (conditioned stimulus); CR (conditioned response); US (unconditioned stimulus); LS (learning set); PGR (psychogalvanic response).

<sup>4</sup> There is no distinction made in this review between concept formation, concept attainment, and concept utilization, not because they do not represent different processes, but because, as of now, these various terms are used to describe experimental procedures that either overlap each other or are not discriminably different. It is likely that, in the near future, operations that clearly distinguish between these processes will become sufficiently standard to warrant separate treatment.

the framework of introspective analysis. It was Hull (58) who, in 1914, began work on a technique that was independent of introspection and aimed at the establishment of quantitative relationships between stimulus and response variables. For him, concept formation was a generalizing abstraction—a process that involved the discrimination of a particular element common to a variety of stimuli. In everyday life, concepts are acquired, according to Hull, without deliberate effort; that is, by trial and error. His experimental technique was intended to be a counterpart of this process. It consisted of the presentation of a series of Chinese figures with a common element in each member which is to be associated with a nonsense syllable. Half of the series was used for evolving the concept, and half was reserved for testing and measuring. The general results are widely known. Human Ss can gradually learn to associate a particular nonsense syllable with a particular, stable element of a changing stimulus pattern. Once this association has been established, it can be transferred to a new stimulus pattern containing the element. All of this S can accomplish without necessarily being able to define the guiding concept.

With this research Hull made two significant and related contributions to behaviorism. He provided an experimental procedure for the study of thinking without recourse to introspection and he offered an analysis of concept formation in terms of S-R relationships without recourse to processes occurring between the observables. Although Hull did not confine his later theorizing to this level, there are several contemporary analyses of concept formation restricting themselves to S-R relationships without postulating any mediating events.

Skinner (116, 117), for example, describes the process as follows: any property of a stimulus which is present when a response is reinforced acquires some degree of control over that response, and this control continues to be exerted when the property appears in other combinations. Behavior may be brought under control of a single property or a special combination of properties of a stimulus while being freed from the control of all other properties. The characteristic result is known as abstraction. The relationship to discrimination may be illustrated by an example. Reinforcing responses to a circular red spot while extinguishing responses to circular spots of all other colors gives the red spot exclusive control over the behavior. This is discrimination because the response has been brought under control of circular red spots but not under the property of redness alone. To achieve the latter, we must reinforce responses to many objects—all red, but differing widely in other properties. Eventually the organism responds only to the property of redness. In this manner Skinner describes a process whose product, he says, is variously called an abstraction, a concept, or a universal, depending on the generality of the referent.

Statistical learning theory is represented in this fold by the work of Bourne & Restle (11). They offer a mathematical model for concept

identification (as they prefer to call it) that is an extension of Restle's account of discrimination learning (104). Two processes are assumed: conditioning to the relevant (rewarded) cues and adaptation to the irrelevant (unrewarded) cues. The rate of learning is assumed to be a function of the proportion of relevant cues and the probability that a cue is present at the time of reinforcement. With this theory, that explicitly treats concept identification as a slight complication of discrimination learning, Bourne & Restle have made and confirmed a number of predictions that will be presented later.

Harlow is another psychologist who gives discrimination a central role in learning set (LS) and, consequently, in concept formation, because he considers the former as basic to the latter. For Harlow (49) LS consists of a discrimination process involving nothing other than the elimination of responses and response tendencies inappropriate to a particular learning situation. This is a uniprocess theory which, unlike Hull's, employs only the process of inhibition and does not require acquisition factors. Among other explanations offered for LS are that provided by Restle (105), which uses a statistical learning theory model, and that by Morrisett & Hovland (90), which conceptualizes LS as the learning of a single class of habits to a complex set of cues. These explanations differ from each other in significant ways, but it is salient for the present discussion that they are all essentially discrimination theories.

Although it may surprise the reader to find him in this company, Gibson (40) is another who stresses discrimination. His explicit hypothesis is that

... for every aspect or property of the phenomenal world of an individual in contact with his environment, however subtle, there is a variable of the energy flux at his receptors, however complex, with which the phenomenal property would correspond if a psycho-physical experiment could be performed.

In perception, discrimination rather than association is the basic process, for Gibson. The phenomenal world is selected, abstracted, and differentiated, not built-up, constructed, or supplemented. Like others, Gibson finds no need to call on intervening processes.

Samples have been offered of various views in which concept formation is dependent on the elimination of responses to irrelevant cues. There are other ways in which these views differ, but it is implicit in these systems that the stimulus equivalence generated in concept formation can be found in some property of the physical stimulus which becomes discriminated from its other properties.

There is another approach to concept formation, and to the study of higher mental processes, that is more prevalent on the contemporary scene. It assumes such a variety of guises that it is difficult to recognize the theme common to all: the focus upon the internal process that mediates between

the stimulus and the response, between the problem and its solution.

The form of this approach most closely related to the discrimination-abstraction theory of Hull is the mediated-response orientation associated with Cofer & Foley (27); Goss (44); Kendler & D'Amato (69); Miller & Dollard (89); Osgood (94); and others. Similar responses to different stimuli (i.e., conceptual responses) are presumed to be primarily attributable to acquired rather than primary stimulus equivalence. Such a view can explain a common response to stimuli in which there is no ostensible physical similarity. Food, for example, is a concept that applies to watermelon and to cornflakes. There is no common stimulus property to these products. What they share is a common response, which may be covert, but can, nevertheless, serve to mediate the final overt response. The early behaviorists were not strangers to the notion of mediation [Meyer (84)]. Watson's motor theory of thinking (131) provided for verbal stimulation that could influence responses to external stimuli. But this source of stimulation had to be made accessible to direct observation before it could be considered scientifically acceptable.

In 1930 Hull (59) formulated the concept of the "pure stimulus act," a response with the sole function of providing the stimulus for other acts. The pure stimulus act or, as it is more frequently called, the cue-producing response is broader than Watson's subvocal speech, since it includes any response the organism is capable of making. Concept formation may take place as a result of acquired stimulus equivalence, based on the acquisition of a common mediating response. This analysis differs from the generalizing-abstraction approach in its emphasis on the significance of the responses brought by the individual to concept formation situations—responses which can generate stimuli that interact or compete with environmental stimuli to control overt behavior.

Although the Gestaltists may not recognize or accept the terminology in which the mediated response approach is framed, their influence must be recognized for the sake of clear historical perspective. Few who have read Koffka's *Principles of Gestalt Psychology* (73) will forget the story of the traveller who crossed what he believed was a snowy plain to arrive safely at an inn, only to drop dead at the feet of the proprietor when he was told that the "plain" was the Lake of Constance. Koffka used this tale to illustrate the distinction between what he called the geographical and behavioral environment, terms that bear clear relationship to external and response-produced stimuli.

Lewin (78) referred to the "psychological" environment which, he said, is not identical with the physical or social environment. He illustrated his point by noting that the same physical object may have quite different types of psychological existence for different children, and for the same child in different situations. A wooden cube may be a missile, a locomotive, or a building block depending upon the total situation. For Lewin, the

psychological environment was to be inferred primarily from the behavior of the individual. It was not, he believed, very fruitful to attempt at that time to relate these mediating responses to the physical environment, a point of view not shared by behaviorists..

The contemporary version of the Gestalt view is expressed by Asch (5) who says,

We act and choose on the basis of what we see, feel, and believe; meanings and values are part and parcel of our actions. When we are mistaken about things we act in terms of our erroneous notions, not in terms of things as they are. To understand human action it is therefore essential to understand the conscious mode in which things appear to us. This is particularly necessary for organisms such as humans whose reactions to the surroundings are so much a consequence of earlier experiences.

Of course, many important differences remain between the Gestalt and behaviorist versions of the mediating response. It is clear, for example, that for the Gestaltist the mediating response is primarily, if not exclusively, perceptual. For the behaviorists the mediating response tends to be primarily verbal. The behaviorists are vitally interested in "tying" the mediated response down at the stimulus as well as at the response end. For the Gestaltists this is either premature, unimportant, or secondary. But there is between them at least the core of an agreement about the importance of the mediating process. Both the similarities and the differences between the liberalized and expanded version of each point of view may be gleaned from the contributions of Bruner (Gestalt) (19) and Osgood (behaviorist) (94) to the University of Colorado Symposium on Contemporary Approaches to Cognition (1957).

There is another important influence on contemporary analyses of the thinking process and, consequently, on concept formation. This stems from the complex mechanical information-processing systems that are serving the cause of science so well in so many instances. It was, of course, inevitable that the "thinking machines" would invite comparison with the human problem solver. Newell, Shaw & Simon (91), for example, present a theory that attempts to deduce the general properties of human thinking from the operation of an automatic digital computer that is programmed for discovering proofs for theorems in elementary symbolic logic. The theory postulates a control system consisting of a number of memories, a number of primitive information processes which operate on the information in the memories, and a set of rules for combining these processes into whole programs of processing. It purports to show how the processes that occur in human problem solving can be compounded of elementary information processes and, hence, how they can be carried out by mechanisms. Since the theory rests on analogies between the human and the mechanical process, Newell *et al.* take some pains to produce comparisons between human problem solving and the behavior of the machine. In this effort



they draw upon previously published descriptions of relevant human behavior. They add nothing to our further understanding of the living mechanisms, but they do provide a better understanding of the computer. Nevertheless, there is a relationship between this theory and what we have been calling the mediating response position; this relationship becomes manifest in the following quotation, which follows an assault on the passivity of simple associationism:

In contrast, we postulate an information-processing system with large storage capacity that holds, among other things, complex strategies (programs) that may be evoked by stimuli. The stimulus determines what strategy or strategies will be evoked; the content of these strategies is already largely determined by the previous experience of the system. The ability of the system to respond in complex and highly selective ways to relatively simple stimuli is a consequence of this storage of programs and this "active" response to stimuli.

Unlike the previously described approaches to the mediating response, which are drawn from empirical observation, this approach draws upon mathematics and computer programming for inspiration.

The use of servo-mechanisms as a model for the analysis of human behavior is echoed by Miller, Galanter & Pribram (88). They laughingly call themselves "subjective behaviorists" because their emphasis is upon the processes immediately behind the action rather than with the action itself. It is ironical that the theories most influenced by cybernetics should be responsible for bringing the circle (spiral?) of historical development back around to where we started, with Watt and the subjective approach of the introspectionists.

It is customary after delineating different theories to summarize the recent research under rubrics related to each position enunciated. This custom has been handed down from the time when psychology was dominated by schools, and when research by one group was not relevant to that by other groups. Such insularity does not seem to be the case for concept formation. One is at a loss to know whether it is because the area is so advanced or so primitive. Perhaps it is because the preliminary analyses consisted of approaches that are neither mutually exclusive nor contradictory. This seems to make possible the classification of recent research in a more general way, making it useful to anyone interested in the area. The rubrics proposed correspond with some of the major sources of variables in psychology: stimulus, motivation-reward, response, and genetic factors. Let it be clear that the breadth of the topic and the limited time and space available have precluded complete coverage of any specified period, although the concentration has been on the last three years (April, 1957 to April, 1960).

#### STIMULUS FACTORS

*Simplicity-complexity.*—It would appear that one of the most accessible stimulus attributes to study would be the simplicity of the concept. This

variable can be measured in two ways. One way is to assay the difficulty of a concept through the response made to it, e.g., by measuring the ease with which it is acquired. This method has been used to compare qualitative differences like abstract and concrete forms, color, and number by Heidbreder and her associates (51 to 54), and by Grant and his associates (46, 47), with somewhat different results. The discrepancies seem to be related to the different experimental procedures used by these investigators to measure concept formation. In a more recent study, Wohlwill (136) documents this interpretation by showing experimentally that the "dominance hierarchy of concepts" will vary with different procedures. He differentiates between abstraction, which he defines as a selective response to a given aspect of the stimulus, and conceptualization, which he considers a process of mediated generalization. Different experimental operations are set up to correspond with abstraction and conceptualization; the data indicate that color and number are more easily abstracted than form, but that form and number are more easily conceptualized than color.

Another way to describe the simplicity of the stimulus is to measure it directly, independently of the response. Such a method is more relevant to the assessment of quantitative differences and lends itself to the establishment of functional relations between measurable aspects of the stimulus and conceptual behavior. In 1952 Hovland (56) described how information theory could be utilized for this purpose. A few years later, Archer, Bourne & Brown (4) used information theory to assess the complexity of stimuli, independently of the dimensions, by measuring the bits of relevant and irrelevant information provided. Complexity was deemed proportional to the amount of irrelevant information. Since the number of possible hypotheses increases in a positively accelerated way as the amount of irrelevant information increases, it was expected that difficulty of concept attainment would increase in a congruent manner. In the two experiments reported, the relationship is more nearly linear up to four bits of irrelevant information and seems to become positively accelerated as the fifth bit is included. Later research [Bourne (8); Bourne & Pendleton (10); Gelfand (39)] finds the relationship to be more linear than accelerated, even with the fifth bit included. The distracting effect of irrelevant stimulation is supported by Bruner, Wallach & Galanter (22), who report that only two instances of "noise" can seriously interfere with the identification of recurrent regularities. Solley & Messick (119) find that the probability of occurrence of a conceptual verbal response is proportional to the number of times the "referent" of the concept occurred.

The additivity of stimulus complexity, along with the variable, stimulus redundancy, is incorporated in the theory offered by Bourne & Restle (11). Redundancy obtains when two or more correlated and binary stimulus dimensions are relevant, or irrelevant, e.g., all triangles are green and all squares are red. Bourne & Haygood (9) find that increases in redundant

relevant information improve concept formation and that the amount of improvement increases with complexity. Redundancy of irrelevant information interferes with performance, but its effect is less inhibiting than comparable degrees of nonredundant irrelevant information.

Concepts may be classified as conjunctive, disjunctive, or relational. In conjunctive concepts, positive instances have two or more features in common, e.g., circular and red. In disjunctive concepts, positive instances have one or the other feature, e.g., circular or red. The relational concept has no common physical elements. Positive instances share common relations, e.g., isosceles triangles of various sizes. Hunt & Hovland (60) found that when Ss could derive a concept on any of these three bases, conjunctive and relational solutions were utilized much more frequently than disjunctive ones.

*Spread of instances.*—The issue here is whether it is more effective to train slightly on a wide variety of instances of the concept or intensively on a few. Hull (58) found that moderate familiarity with each member of the series from which the concept is to be drawn is more efficient than twice as thorough familiarity with half as many cases. On the other hand, research on LS that holds constant the total number of trials while varying the number of trials per problem finds that when three and 12 trials per problem are compared [Levine, Levinson & Harlow (77)] and when one and 12 trials per problem are compared [Levine & Harlow (76)], no difference obtains. Within the limits tested, it does not matter whether the number of instances is relatively wide or relatively narrow, or whether there are few or more trials per problem; only the over-all number of trials is important.

A related contradiction appears in Adams' finding (2) that intensive single-problem training produced more transfer, than a less intensive multiple training procedure, whereas Callantine & Warren (26) find that the greater the number of training problems, the greater the transfer.

One explanation of these discordant results is suggested by Morrisett & Hovland (90). Transfer on LS is presumed dependent on two separate factors: learning within the problem and generalization between the problems. The first factor, learning within the problem, depends on the mastery achieved within any one problem; therefore, the more learning, the more transfer. The failure of previous research to show any positive effects of the number of trials per problem suggests that short of attaining some criterion of learning, this factor may not be present in sufficient strength to produce any effect. The second factor, generalization between the problems, is strengthened by an increase in the number of problems. Therefore, training which allows both factors to operate, i.e., a high degree of learning on several problems, should, and in their research does, produce the most efficient learning-to-learn. Similar but statistically insignificant results were obtained by Pubols (100).

*Stimulus element vs. relationship.*—The somewhat tattered controversy over what the true nature of the stimulus is weaves wearily through a few studies that seem to be appropriately classified under concept formation. Gonzalez & Ross (42) report that children between the ages of three and five years can acquire the concept of middle size when tested with an intermediate set equidistant from the two training sets, despite their inability to verbalize the basis of solution. These results are interpreted as refuting Spence's theory of transposition (120, 121) and Kuenne's (75) contention that preverbal children function on a level qualitatively distinct from that of verbal children, and as favoring the response to stimulus relationships being primitive rather than derived.

On the other hand, Brown, Overall & Gentry (18) find that monkeys trained and then transposed on the middle-size problem could learn both relationship and absolute stimulus values. However, the tendency to respond on the basis of absolute values (nonconceptually, to coin a neologism) is the stronger tendency, and relational properties (response to the concept of middle size) is employed as a basis by their Ss only when differential absolute values are lacking.

Royer (109) compares the formation of auditory concepts based on a recurrent identical auditory stimulus element (a prominent partial in a harmonic complex tone) with concepts based on a recurrent relationship (the harmonic structure of a complex tone). No differences in transfer are found for the two conditions after the learning of five lists. The author's conclusion that there is no difference between these two stimulus conditions is doubtful since the number of correct identifications is so low that it is questionable whether any concept was attained in either group.

Ray (101) finds that adult humans, learning to solve problems composed of a series of digits by one pattern (e.g., "plus one" as in 345 or 678) leads to positive transfer for other patterns (e.g., "plus two" as in 357 or "minus one" as in 876). This is interpreted as showing the occurrence of stimulus generalization among meaningful stimulus patterns.

Although these studies do not particularly clarify the issue of stimulus element vs. relationship, they do show that concepts dependent on relationships can be acquired by monkeys and children as well as by adults.

#### MOTIVATION AND REWARD FACTORS

*Motivation.*—There are relatively few studies of the effect of motivation on concept learning or performance. This is understandable since most concept formation studies deal with human subjects, whose drive level cannot be easily manipulated. Here, as in human learning, the Taylor Manifest Anxiety Scale (MAS) is used in an attempt to measure, and thus differentiate, what cannot be directly manipulated. Mednick (82) found extreme MAS scores (high drive) positively related to the magnitude of mediated generalization responsivity. Romanow (108) reports two ex-

periments. In one, three groups of Ss differed in level of manifest anxiety; in the other, three groups were given differential instructions in order to induce three different degrees of ego-involvement. All Ss were given a concept-formation task in which the associative strengths of the correct response and of competing tendencies were varied. Prediction from a mediated-response-habit-hierarchy theory was that high drive groups should be inferior to low drive groups when the correct response was weak relative to the incorrect response. It was confirmed; however, the expected parallelism between anxiety and ego-involvement was not obtained. Both of these studies show that drive operates on mediating responses in the same way as on overt responses.

*Reward or feedback.*—One of the most popular parameters of reinforcement in today's psychological literature, not excepting that on concept formation, is scheduling. Green (48) used an operant stimulus discrimination procedure to find that the extent of concept formation was inversely related to the ratio of responses to reinforcement. Bourne & Pendleton (10) varied the feedback probability (70, 80, 90, and 100 per cent) on a four-choice concept identification task and found an inverse linear relationship between errors and feedback probability. Rhine & Silun (107) confirmed the positive relationship between consistency of reinforcement and efficiency of concept formation, and added the observation that resistance to change is, as might be expected, least for continuous reinforcement conditions. These three studies varied the reinforcement or feedback of the relevant concept. Gormezano & Grant (43) tested the effect of intermittent reinforcement on the irrelevant concept and found that the difficulty of learning the relevant concept increases with the degree of reinforcement of the irrelevant concept.

*Preliminary training.*—Preliminary training on a given dimension, e.g., form, yields positive transfer when subjects are shifted to new and different stimuli within the same dimension [Bensberg (6)], even when the shift involves a reversal of previously reinforced responses [Kendler & D'Amato (69)]. These results are interpreted as evidence that the relevant mediating response has become functionally dominant.

*Positive and negative instances.*—Hovland & Weiss (57) utilized information theory to equate the amount of information provided by positive and negative instances of the correct concept, and found that more Ss attain the correct concept when it is transmitted by all-positive instances than by all-negative instances. However, over half of the Ss were able to arrive at the correct concept on the basis of negative instances only. Mixed positive and negative instances are intermediate between all-positive and all-negative series in difficulty of learning. When negative instances are displayed simultaneously, the accuracy of concept attainment is higher than when they are presented successively. A more recent study by Cahill & Hovland (25) confirms and elaborates on this last observation. It points

up the important role of memory in the acquisition of concepts that involves the successive presentation of instances, negative in the present case. They find that a very low proportion of errors is attributable to failure to assimilate the information perceptually available or to failure to draw appropriate inferences concerning the concept being sought. More of the errors are due to failure to remember earlier instances in such a way as to see their implications, and these errors increase progressively with increasing numbers of intervening instances. Such an observation is of particular significance in the simulation of cognitive behavior by high speed computers, since the machines have unlimited memories.

#### RESPONSE FACTORS

The range of responses employed in concept formation is very wide, varying from the rate of lever pressing to complex verbal introspection. Although it may yet prove a profitable venture to analyze them all, the present review will be more limited. Comments will be confined to two response provinces. One of these, the verbal response, has been subjected to much recent scrutiny. The other, which is less well developed but nevertheless very important, consists of efforts to analyze the various steps involved in the process of concept formation.

*Verbalization.*—It is inevitable that a review of concept formation should touch upon the role of language, a system of responses so important it has given rise to a vigorous science of its own. Since psycholinguistics was covered in the 1960 *Annual Review* by Rubenstein & Aborn (110), only a few comments will be made here. The dependence of concepts on language is represented in its most extreme form by Whorf (134). For him all higher levels of thinking involve language. The structure of this language, in turn, determines man's thoughts, concepts, and view of the universe. The harmony of Whorf's view with Watson's motor theory of thinking, including its de-emphasis on perceptual determinants, is obvious. It seems to be the consensus that experimental results support the more moderate position that concepts are influenced both by perceptual and linguistic variables.

Nevertheless, it is generally admitted that language responses are particularly important, for one thing because verbal stimuli and responses so readily engender conceptualization. This line of thought is illustrated in the literature on semantic generalization, in which there is ample demonstration that different words or objects and their names (i.e., physically different stimuli) often produce a common response. Cofer & Foley (27) have supplied a well-integrated review of this literature. More recent work by Cofer & Yarczower (28) ascribes the ability of synonyms to evoke common responses to their associative strength. Branca (14), who tested for semantic generalization using shock as the unconditioned stimulus (US) and psychogalvanic response (PGR) as the conditioned response



(CR), gives unwitting support to their position. He found that, when he compared several conditions, the most semantic generalization occurred when CS consisted of three synonyms and the test stimulus was a fourth synonym. There was less generalization when the CS was a picture and the test stimulus was its name; when the CS was the name and the test stimulus was a picture; and when CS was one word and the test stimulus was its synonym. It is the author's claim that his results demonstrate that semantic generalization is dependent on S's awareness of the connection, because Ss who displayed semantic generalization indicated awareness. However, since awareness was ascertained after the test for transfer, it is possible that they became aware of the connection after it was established. Under these conditions awareness would be more epiphenomenal than requisite.

Soviet psychology has been particularly interested in this area. Simon (115) points out that Pavlov, late in his career, developed a theory of the second signalling system. The first signal system refers to that part of Pavlov's work which deals with stimulation from the external world. But man, he said, unlike other species, is an articulate being who produces words that can also act as stimuli. Such stimuli, which comprise the second signal system, differ qualitatively from others because they comprehend, generalize, and stand for the multitude of separate stimuli of the first signal system. Some of the earliest studies in which a conditioned reaction was established to a stimulus object and generalization was obtained to its name, and vice versa, were by Soviet psychologists. The Russians are, apparently, still working in the area, and what they are doing will be sampled in the work of Liublinskaya, Luria, Slavina, and Galperin in various sections of this review. According to Liublinskaya (79) a word becomes a signal of the second system only when it becomes a concept. He puts it this way:

There is . . . a change in the meaning of a familiar word. Initially, the word is connected with one concrete object, having a number of particular characteristic features which belong to it alone (e.g., white spots on a pink cup); the same word later signalizes features common to a multitude of objects which, though different in many ways, constitute a single group. While at first the word "cup" signalizes a combination of features belonging to the object fortuitously as well as essentially, later the word signalizes the essential feature of the object, irrespective of changes in secondary features. This essential feature, found in the concrete and single instance, remains unchangeable and common for all objects of the same kind and is the means of distinguishing them from other similar classes of objects. Only at this stage has the word acquired for the child that "comprehensive" character distinctive of signals of the second system which cannot be compared either quantitatively or qualitatively with the conditioned stimuli of animals (Pavlov), i.e., with signals of the first system.

Liublinskaya also reports that learning of concepts is facilitated and conceptual transfer is extended by the use of verbal labels. Moreover,

applying a verbal label to a relevant dimension is more effective than prior training on the dimension. He also provides some support to the Whorfan hypothesis since he claims that the development of perception is inseparably connected with the intervention of the second signal system.

Some comparisons are being made between the various modalities in which language may be expressed. Ivaschenko (62) describes two investigations of the relationships between words heard, seen, and pronounced by the subject. In one study the CS was the heard word "green," and transfer was tested to the word "green" presented visually. Considerable positive transfer was obtained. In the second study the CS was the pronounced word, which S was instructed to repeat every 10 or 15 seconds. Under these conditions there was practically no transfer to the seen and heard word. Apparently there is more mediated generalization from the heard word to the pronounced word than vice versa. The author suggests that this result is compatible with the fact that it is easier to learn to understand words than to speak them.

Back on the American scene, Underwood & Richardson (128) have concretized the concept of habit family hierarchy of verbal responses by providing a measure of the frequency with which a list of 213 words evoke, in a word-association context, particular verbal associations. The associations were restricted to those descriptive of sensory impressions, e.g., round, red. Their purpose was to provide a list that could serve as a basis for research in concept formation. The list was used first by Underwood & Richardson in a study equating frequency with dominance in the response hierarchy (129). They found, as expected, that concept learning was positively related to dominance and negatively related to the degree with which associations overlapped. Freedman & Mednick (36), using a procedure adapted from Underwood & Richardson, kept mean dominance of the words from which the concept was to be attained equal, but varied the variance (e.g., low variance consisted of uniformly moderate dominance words; a high variance list consisted of both high and low dominance). High variance concepts were learned more rapidly; a result that is consistent with a learning-theory-mediated-response approach. Kaplan (65) compared the effectiveness of mediated verbal associations derived prior to the experimental situation, as drawn from Underwood & Richardson's list, with those developed in the experimental situation by means of a paired associates learning technique. As Jost's law would predict, the preformed associations were more effective.

Another major research trend has been generated by Osgood's formulation of the conceptual process (94). He deals with meaning through a representational mediation process in which a "nonsignificate" stimulus (the US) becomes associated with a "significate" stimulus (the CS) when accompanied by reinforcement. The CS comes to evoke a fractional portion of the total behavior elicited by the significate, which becomes its meaning

and, for present purposes, the basis for conceptual generalization. This meaning can be scaled with the use of the semantic differential [Osgood, Suci & Tannenbaum (95)]. The heuristic value of this approach is attested to by previous annual reviews (37, 68, 110). Among the most recent work using the semantic differential are the several studies by the Staates and their co-workers. They have developed a classical conditioning procedure that successfully transfers evaluative responses elicited by words to contiguously presented neutral stimuli (125). The neutral stimuli they experimented with included nonsense syllables, masculine names, and nations (122). Their earlier research was done with groups of Ss and, recently, they were able to replicate the effect on individual Ss (123). Another study (124) showed that evaluative "meaning" conditioned to a word (e.g., rug) applied also to its synonym (carpet), another demonstration of semantic generalization. Rhine (106) has developed what he calls a concept-formation approach to attitude acquisition, in which an attitude is defined as a concept with an evaluative dimension. He employs the mediated-response approach to explain attitude learning, thus extending the scope of the theory to encompass social phenomena.

Although the value of the semantic differential as a research tool is considerable, Norman (93), who has recently investigated its stability characteristics, concludes that individual ratings show rather low stability when remeasured after a four-week period. Group ratings over the same period show rather high stability.

Another healthy line of research produced by consideration of the mediating function of language in concept formation consists of studying the effect of associating verbal responses to stimuli that will later be used in concept formation tasks. The theory is that the verbal responses become covert and as such serve to mediate overt responses. Similar labels facilitate concept formation and dissimilar labels facilitate discrimination. The validity of this phenomenon seems to be sufficiently well established for the more recent investigators to be concerned with ascertaining some of its parameters. Studies have shown that the effectiveness of verbal mediation in discrimination learning or conceptual sorting is dependent on the degree of mastery [Goss & Moylan (45); Yarczower (137)] and the distinctiveness [Norcross (92)] of the previously acquired discriminative verbal responses. Fenn & Goss (33) found that there was no difference in the effect of familiar words and nonsense syllables; nor did they find any differences between schizophrenic and normal Ss in their responsiveness to the effects of verbal mediation. The general advantage of this approach is that it subsumes concept formation under already established principles of psychology, a very desirable economy.

*Process.*—The idea of a process is inherent in the mediation approach to concept formation since, at a minimum, it requires one S-R link to intervene between the external stimulus and the overt response; therefore,

a full consideration of the conceptual response must involve a process rather than a single response.

Another approach has been reported by Flavell & Draguns (34) along with a history of "microgenesis." Microgenesis is "the series of events presumed to occur in the course of a single, brief conceptual or perceptual act." Their theory is based on the proposition that this series of swiftly occurring acts, of which the individual is not necessarily aware, constitutes a microdevelopmental pattern. With their co-workers (35) they have also provided an experimental test of one of their hypotheses, namely, that in word association there are covert word responses pushing for expression early in the associative process and that these early microgenetic forms are less logical, more paleological, than later ones. It is assumed that when normal individuals associate in an unhurried manner, the immature responses are repressed, the microgenetically later ones being the ones actually spoken; therefore, forcing rapid responses should increase immature associations. Adequate test of this hypothesis involves clear delineation of mature and immature responses, a whole research area in itself. It is not surprising that only partial confirmation of the prediction was obtained.

A more logical, rather than psychological, approach to the analysis of process in concept attainment is provided in the program of research offered by Bruner, Goodnow & Austin (20). They presented subjects with an array of instances from which to select the relevant concept. For each instance a decision was made and subsequently validated. The sequence of decisions was regarded as a strategy which embodies three general objectives: to maximize the information gleaned from each instance, to keep down "cognitive strain," and to regulate the risk. The several kinds of strategies yielded by this analysis and conditions that lead to their adoption have been reviewed by Gagné (37). The necessity of, at least, mentioning it here arose from the fact that it represents the most highly organized work on the analysis of process in recent literature.

A more modest effort is presented by Johnson (63), who aims to describe problem-solving activities in functional units that are larger than single responses and smaller than the whole problem-solving episode. To keep things manageable, he starts with a two-part analysis; the first part is called "preparation" and the second "solution." The experimental method divides the presentation of the problem into two parts that are presented in a serial exposure box. The first part, preparation, provides some information, but solution becomes possible only upon presentation of the second part. If Ss are allowed to switch the exposure from the preparation part to the solution part themselves, it becomes possible to measure the length of time they spend on each part and the number of times they switch back and forth. Among Johnson's results is the finding that, when the amount of preparatory material is considerable, or when the formulation derived from the preparation does not fit available solutions, the thinker will switch back

to preparation and be more successful on the next try. He also corroborates previous observations that, in a concept formation task, when preparation favors one dimension, a set is produced so that solution is slanted in the same direction.

Two Soviet psychologists, working closely with the educational problems of "unsuccessful" first-grade pupils, have come up with an analysis of the acquisition of arithmetical concepts that has both practical and theoretical interest. Slavina (118) evolved a series of steps enabling the children to learn what they could not learn in the ordinary classroom. Galperin (38) formulated the process as follows: (a) Teacher provides initial orientation to relevant stimuli. (b) Child learns to make relevant physical responses with concrete objects, e.g., counting with sticks or arranging cut-out numbers in order. (c) Child "masters the action on the plane of audible speech." The goal is to replace the concrete operations with verbal operations. The physical props are removed very gradually and the child is allowed frequent returns to the previous level. (d) Child "transfers the action to the mental plane," i.e., part of the response chain becomes covert. This too is accomplished gradually by first teaching the child to whisper. (e) "Consolidating the mental action," which seems to mean dropping out the unnecessary verbal concomitants of necessary overt responses, by virtue of the mechanisms of the anticipatory goal response. This makes available immediate solutions to problems that may have originally involved many laborious steps. Galperin notes that, at this stage of conceptualization, introspection does not reveal the course of the process, either because the covert responses may be so abbreviated as to be unrecognized, or because they have disappeared.

Presumably, this process can be generalized to normal children and to other concepts, but in these instances some of the steps take place so quickly that they go unrecognized. It is an interesting and productive idea that retarded children can help us to better understand a process because they need to have it slowed down to the point of recognition.

#### GENETIC FACTORS

*Phylogenetic considerations.*—The question of whether animals can acquire concepts has been replaced by questions about what species can acquire what concepts, and under what conditions. For example, besides the well-known capacity of monkeys for delayed response, learning-to-learn, etc., there have been new demonstrations of their ability to respond on a rather high conceptual level. Monkeys can learn to respond to novelty, i.e., to a new member of a stimulus pair coupled with an old member that was either previously positive or negative [Brown, Overall & Blodgett (16); Brown, Overall & Gentry (17)]. They can learn to respond to "seriality," i.e., the order in which the stimuli are presented [Massar & Davis (81)].

Phylogenetic comparisons have been of particular interest in the study

of higher mental processes, but have not approached fulfillment, because of the lack of development of a vehicle applicable to the phylogenetic gamut. Delayed response techniques have some of the qualifications and are still employed [Miles (85); Petrinovich & Bolles (96)], but more directly related to concept formation per se, is the methodology of learning set. Object-quality learning sets have been compared for cats [Warren & Baron (130)], raccoons [Johnson & Michels (64); Shell & Riopelle (112)], and for several species of monkeys [Miles (86); Miles & Meyer (87); Shell & Riopelle (113)]. The consensus appears to be that LS can be established below the primate level, but the ease of establishment and the ultimate efficiency of performance increase as the phylogenetic scale is ascended. Some rats [Koronakos & Arnold (74)] and some cats [Boyd & Warren (13)] have shown positive transfer in successive oddity problems. But their performance is inferior to primates.

Reversal learning, although ostensibly in the domain of simple learning, has at least one foot inside the border of concept formation. One reason for this straddling is that learning easily to do the opposite of what one has previously done implies a level of abstraction more congruent with concept formation than with simple S-R association. Piaget (99) puts it this way: higher order mental operations

are reversible as against simple actions which are irreversible. In this way, the operation of combining can be inverted immediately into the operation of dissociating, whereas the act of writing from left to right cannot be inverted to one of writing from right to left without a new habit being acquired differing from the first.

Our consideration of reversal will deal first with successive reversals. It has been satisfactorily demonstrated that primates, after initial negative transfer, display a progressive increase in the efficiency of reversal, although the terminal accuracy of marmosets is lower than that of rhesus monkeys in a comparable situation [Cotterman, Meyer & Wickens (29)]. As we go down the scale, we find that a high level of efficiency in position discrimination reversals was obtained with rats [Pubols (100)]. Reid (103) concludes that his attempts to obtain a reversal learning set in pigeons provides no evidence of such capacity. Wodinsky & Bitterman (135), working with fish, find that initial interference caused by reversal gradually decreases, but first day's trials never show positive transfer. The lowest phylogenetic species studied in the recent literature is the invertebrate isopod. Thompson (127) concludes that isopods "do not show the characteristic improvement in reversal performance that has been demonstrated for lower vertebrate forms, such as newts and turtles."

The conditions under which these various species have been tested differ widely. A more careful comparison may turn up some discrepancies, but in the light of present data it looks as though the successive reversal procedure provides a good vehicle for differentiating the species and, consequently, for the study of higher mental processes.



Along with the interest in successive reversals, a literature has grown up about one-trial reversal learning. When animals are reversed before or at criterion, negative transfer results. If there is, however, a considerable amount of overlearning prior to transfer [Pubols (100); Reid (102)], combined with a moderate degree of motivation [Bruner *et al.* (21)], rats will show either positive transfer or no transfer in a position-discrimination reversal. Whether this behavior is related to concept formation or is to be explained by simple S-R mechanisms is still at issue [D'Amato & Jagoda (30)].

Another branch of research has grown up around comparisons between reversal and nonreversal shift in rats, adult humans, and children. Reversal shift requires S to make a different overt response to a previously relevant concept. Nonreversal shift requires a shift to a new concept. In brief, the results are that for rats reversal shift is more difficult than nonreversal shift [Kelleher (67)]. This is consistent with a simple S-R formulation of discrimination learning. In adult humans the opposite result obtains; reversal is relatively easy [Buss (24); Harrow & Friedman (50); Kendler & D'Amato (69)]. In order to account for this behavior within an S-R framework, a mediating link between the external stimulus and the overt response must be postulated. Comparable research with young children showed that children under five tended to respond predominantly in an unmediated manner, i.e., like the rats [Kendler, Kendler & Wells (71)]. Children between five and six were at a transition point where roughly half of them responded conceptually and half did not [Kendler & Kendler (70)]. This research leads us neatly to the next rubric.

*Ontogenetic factors.*—There are three major lines of influence here which go their ways with relatively little interaction. One of these lines stems from Piaget's laboratory, another from the analysis of the role of language, and a third from the methodology of learning set.

Piaget has spent over 30 years in pursuit of an understanding of the development of thought, or as he sometimes calls it "conceptual operations," in children. The fruit of this long and prolific effort is a philosophically sophisticated theory that encompasses intellectual development from infancy (97, 98) to adolescence [Inhelder & Piaget (61)]. The theory is based on a long series of seminaturalistic observations of the behavior of children in response to ingenious experimental situations and adroit questioning. It makes considerable use of symbolic logic and other mathematical systems to describe and order the empirical observations. In general, the findings of Piaget, and Inhelder, seem to be that intellectual development can be characterized as a series of stages in which each stage lays the foundation for its successor. There are four main stages. The first, which reaches from birth to two years, is characterized by sensorimotor development. During this stage the child learns that objects have permanency even when removed from his perceptual field. In the second stage, which covers

from two to seven years, the child can use language and the internalization of actions becomes possible. Symbolic function appears but there is an absence of both "reversible operations" and of the concepts of conservation of quantity, size, etc. (e.g., the child is apt to say that five checkers placed far apart are more than five checkers placed close together). In the third stage (7 to 11) the conceptual operations of the child include the ability to perform concrete operations that belong to the logic of classes and relations but do not yet take into account their combinatorial possibilities. The fourth stage (11 to 15), which leads to adult logic, is marked by the appearance of the ability to reason by hypothesis. The logic is now concerned with propositions as well as objects. Movement from one stage to another is determined by a combination of experiential and maturational variables, related to, but not exclusively determined by, age.

More specific findings are both so numerous and so theory bound that it is impossible to present them in the context of this review. Let it suffice for the present to point to the increasing interaction of Piaget and his collaborators with American investigators in the expectation that this will yield considerable mutual benefit. Among the possible advantages for Piaget's laboratory is an increase in both the rigor of experimentation and clarity of theorizing. Their American counterparts can learn how to increase their scope and boldness.

The second line of influence has as wide a ken as the first and yet must be treated in the same pithy manner. It stems from the belief that as children get older their behavior comes increasingly under control of the stimulation they themselves engender. Their own verbal responses are a most important source of such self-stimulation. This is the position of Luria (80) who takes off from Pavlov's second signal system. His observations and those of his colleagues lead him to conclude that, at the outset, verbal behavior has as its primary function, communication. As the child matures, the verbal behavior, which may be implicit or explicit, gradually comes to mediate and regulate overt behavior. Behavior that is conditioned without verbal associations is relatively unstable, is dependent on constant reinforcement, and disintegrates at a slight change in the manner of presenting the signals. Behavior that is conditioned via verbal associations is quickly acquired, very stable, and generalizes widely. Normal humans from 5½ years of age and on tend to operate via verbal control, whereas younger children and mental retardates do not. The implications of this position are as interesting as they are congruent with the mediated-response analysis. However, not all the data supporting it are available in English; corroborative work needs to be done.

In the United States, research is not as organized, but there does seem to be a rising interest in assessing the relations between language and concept development. To supply the flavor of the research the following few scattered studies are briefly reviewed. Burstein (23) finds that third-grade

children confuse antonyms with synonyms more often than sixth graders. He makes the interesting suggestion that this confusion between opposites and similarities may be a step in the evolution of concepts. Bousfield *et al.* (12) discovered that in the recall of a list of words, the amount of associative clustering (i.e., tendency to recall a list of words in groups that share a common meaning, concept, association, perception, etc.) and the total amount recalled increased with age.

Rudel (11) finds no significant difference in the transposition response of verbal and preverbal children for children under 45 months; verbal and preverbal are defined in terms of the ability of the children to respond to relevant verbal questions. She points out that although these results may appear to be in conflict with those of Kuenne (75) and Albert & Ehrenfreund (3), they are actually not so. The "verbal" Ss of Rudel's study were the same age as the children that these other investigators called "preverbal." The implication of this result is that in the earlier studies, verbal and preverbal were used more as explanatory constructs than as descriptive labels. One interpretation of all of these results is that, as Luria would have it, the issue is not whether the children can say the relevant words; it is whether the words control their behavior. The children of Rudel's study were all under 46 months, too young for verbal control on Luria's developmental schedule. Kuenne's "verbalizers" were predominantly five- and six-year-olds.

It is appropriate to mention at this point the work of Werner & Kaplan (133), who offer a line-drawing technique for the study of a response "bordering on the gesture." This is of interest here because of their suggestion that the gesture can supply a primitive form of mediation which developmentally precedes verbalization.

Brown (15) takes issue with the commonly accepted notion that development proceeds from the concrete to the abstract. He claims that although there are more generic terms in adult language, this does not necessarily characterize the thinking involved. Children use more concrete terms because they are taught to do so and because such terms have greater utility in their lives. If generalization refers to failure to discriminate, then children are quite prone to abstract. This observation is confirmed by Mednick & Lehtinen (83), who find that simple stimulus generalization decreases with age. In the sense of generalization after discrimination, Brown argues, children are less abstract than adults. Therefore, if there is a progression from children to adults (or from animal to man), it is to be measured by the number of differentiated subordinates to a category rather than by the category range. This is a clarification of terms that can lead to profitable research.

The third line taken in recent developmental studies is the least theoretically developed but, perhaps, the most well represented. It involves the extension of learning-set methodology to comparisons between children of different chronological and mental ages. Harlow (49) cites evidence that

chronological age is a variable influencing LS in rhesus monkeys. The general results with human children are that normal preschoolers quickly and easily form learning sets on successive discrimination problems [Shepard (114)]. Retardates, depending on the problem and their mental age, also acquire learning sets, e.g., House & Zeaman (55) showed that even imbeciles approached one-trial learning after successive position reversals. They remark that the performances of these children resembled those of rats in comparable situations. All of the following studies testify that the ability to form learning sets is positively correlated with mental age: Ellis (31); Ellis & Sloan (32); Girardeau (41); Kaufman & Peterson (66); Koch & Meyer (72); Stevenson & Swartz (126). Such consistent results over a wide gamut of tasks and by many independent investigators is an event psychology cannot afford to overlook. It adds considerably to the value of LS as a research tool for the collection of systematic data, on the one hand, and as a sensitive measure of ontogenetic development on the other.

## CONCLUDING COMMENT

Interest in the area of concept formation is lively and intelligent. The research is good; yet somehow, the progress appears small. Perhaps, when we deal with complex processes that are concealed from direct observation, we must move slowly. Even so, the outlook is hopeful. Systematic theorizing and rigorous research are beginning to emerge. With them are coming some insights into the operation of conceptual process that hold promise for the emergence of the clearly defined, encompassing principles so badly needed.

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## BEHAVIORAL DECISION THEORY<sup>1,2</sup>

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In 1954 I published a review article (53) covering the psychological and economic theories of riskless and risky decision making, the theory of games, and the experiments relating to these theories. This review covers the same subject matter for the period 1954 through April, 1960. Like the previous review, it is confined to work on human subjects.

Several books which include reviews and several review articles have appeared since 1954. Most important among them is *The Foundations of Statistics*, by Savage (106). This brilliant and entertaining book is mostly about subjective probability (see below), but it begins with a thorough, profound discussion of decision making. Second in importance is the multi-authored book, *Decision Processes* (129). Some of its chapters were reviewed in 1954 on the basis of unpublished drafts; others are reviewed later in this chapter. The third major book, *Games and Decisions* by Luce & Raiffa (88), is primarily concerned with game theory but also covers most of the rest of the field. Williams' book *The Compleat Strategyst* (138) is certainly the cleverest popular exposition of game theory. Shubik (111) has defined some of the terms of risky decision theory, Bates (6) has discussed the philosophy of decision making, Arrow (2) has reviewed utility theory, as has Ellsberg (59), and Vajda (134) has summarized the mathematical content of game theory. Three more or less elementary expositions of riskless and risky decision theory are offered by Marschak (89, 90, 91). Wasserman & Silander (136) have prepared an annotated bibliography of decision-making literature which is extraordinary for omitting most of the literature on risky decision making in both psychology and economics; it focusses, instead, on the rather extensive economic and business literature on how entrepreneurial decisions are actually made. Riley & Young (102) have prepared a bibliography of war gaming. Simon (119) has reprinted a number of his articles, some of which are reviewed below, in a book. For those who are more interested in what men should do than in what they do do, Chernoff & Moses (17) have published an elementary text on statistical decision theory, thus making accessible to the nonmathematician the ideas which Blackwell & Girshick (9) so elegantly present to mathematicians only. Schlaifer (108), in a brilliant elementary textbook whose

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<sup>2</sup> Abbreviations used in this chapter include: *SEU* (subjectively expected utility).

words (and equations) are for children but its meaning for men, goes much farther than any other textbook writer in demonstrating the necessity of abandoning the traditional Neyman-Pearson formulation of statistical problems, at least for business decision-making purposes, in favor of a statistical decision-making approach derived primarily from Savage (106) which places very heavy emphasis on subjective probabilities. Unless someone like Schlaifer leads psychological statistics out of the wilderness of  $t$  and  $F$  tests, psychological experimenters may find themselves still seeking the magic .05 level when to statisticians and other applied scientists the notion of significance level is only a historical curiosity.

#### STATIC MODELS AND RELATED EXPERIMENTS

In the tradition which began with Daniel Bernoulli in 1738 and was firmly fixed by economic theory and by Von Neumann & Morgenstern's classic book (135), theories of decision making are static theories. They are concerned with the determiners of a single choice among courses of action, rather than with a sequence of choices. Since any choice is imbedded in a sequence of other choices, any static model can be, at best, only a first approximation. Nevertheless, in 1960, as in 1954, most theoretical and experimental work centers on a single static model. Why? The static models work, at least to some extent, and the theoretical and experimental difficulties become unmanageably complex in most dynamic cases.

In 1955 I listed four models for static risky decision making, defined by four equations (56). The four models had in common the notion that a quantity can be obtained by taking for each possible outcome of a given course of action a number representing the value of the payoff and a number representing the probability of obtaining that payoff, multiplying the two, and then adding across all possible outcomes of the course of action. All four models assert that a decision maker behaves as though he compares these sums and chooses the course of action from among those available to him for which the sum of probability-value products is largest. The models differ in that the measure of value can be objective (in dollars, or some similar physical measure) or subjective (subjective value is usually called utility), and the measure of probability similarly can be objective or subjective; four combinations of these possibilities exist. No existing behavioral model seriously asserts that people attempt to maximize the sum of products of objective value and objective probability; too many data disprove the assertion. [But see (128).] The combination of subjective value or utility and objective probability characterizes the expected utility maximization model; Von Neumann & Morgenstern (135) defended this model and, thus, made it important, but in 1954 it was already clear that it too does not fit the facts. Work since then has focussed on the model which asserts that people maximize the product of utility and subjective probability; I have named this the subjectively expected utility maximization model (*SEU* model).

No one since 1954 has defended the subjective probability-objective value model; this review will ignore it.

#### RESEARCH ON UTILITY

In 1954 there was general agreement on what utility is and that it can be measured, but no real agreement on how to measure it. In 1960 there is at least a conceptually adequate method of measurement (assuming a *SEU* model)—but some doubt about whether a *SEU* model, and therefore any measurement methods based on it, can stand up to the facts. This section will assume the validity of a *SEU* model; arguments against that assumption will be considered in the section on variance preferences.

*Direct estimation.*—The most direct way of finding out how valuable \$10 is to someone is to ask him. No one has quite done that, but Stevens (121) reports anecdotally the results of a semi-experiment in which Galanter asked Ss how much money would be twice (or half) as desirable as \$10, and other amounts. He found results consistent with Stevens' general power law for psychophysics (120), with an exponent of about 0.5, which implies decreasing marginal utility. In the absence of more detailed information, it is impossible to tell how seriously to take the results. If direct psychophysical methods will, in fact, yield a nonlinear utility scale, and if that scale then turns out to be useful in predicting decisions, clearly they are preferable to any indirect methods. But it would require a lot of evidence to convince many researchers that subjects, asked how much money is half as desirable as \$10, would systematically answer something different from \$5. The numerical properties of money may be far more important in determining responses than their utility properties, no matter what instructions are given.

*Measurement via the SEU model.*—Several attempts at indirect measurement of the utility of money (or other valuable objects) via the *SEU* model have been made. All such attempts face the difficulty that choices among bets, the raw data from which utilities must be inferred, may be determined in a *SEU* model both by utilities and by subjective probabilities; two mechanisms, one logically sound and the other not, have been proposed to disentangle these two determiners. I presented the unsound solution in 1955 (56; see also 55). It consisted of assuming that the utility of  $N$  identical bets is equal to  $N$  times the utility of one such bet. This assumption makes possible the factoring of subjective probabilities out of the equations for the *SEUs* of certain bets and so permits the direct inference of utilities. The logical difficulty is that the assumption, if taken in full generality, implies that utility is linear with money; special further assumptions are necessary to avoid this implication. The results of the experiment based on the assumption were utility curves which were generally linear in the positive quadrant, but nonlinear in the negative quadrant (losses). Internal consistency checks partially verified the  $N$ -bets assumption which may con-



sequently have merit as an empirical truth, though not as a logical foundation for measurement of utility.

In 1957 Davidson, Suppes & Siegel (42) published a monograph presenting the only satisfactory utility measurement technique that has yet been exploited. This technique, which is based on a suggestion by Ramsey (100), depends on the prior identification of an event whose subjective probability of occurrence is equal to its subjective probability of nonoccurrence. After trying coin flips, penny matching, and several other possibilities, Davidson, Suppes & Siegel ended up using a six-sided die, with the nonsense syllable Z EJ printed on three of its sides and ZOJ on the other three (other nonsense syllables were also used, with the same results). The criterion of equal subjective probability for Z EJ and ZOJ was that the subject did not care (in a precisely defined behavioral sense) which event was associated with the more favorable outcome of a two-outcome bet.

Once such a subjectively 50-50 event has been identified (and provided that it remains subjectively 50-50 regardless of what outcomes are paired with it and regardless of experience with it), equally spaced utility intervals can easily be determined since the subjective probabilities, all being one-half, cancel out of the equations for *SEUs*. However, Davidson, Suppes & Siegel resist defining indifference as interpolated 50 per cent preference, and so, instead, determined upper and lower bounds on their subjects' utility functions. These bounds were generally close together; Davidson, Suppes & Siegel had unusually good luck in inducing their subjects to be consistent. Some of the functions look fairly linear; others do not. Davidson, Suppes & Siegel [see also Davidson & Suppes (41)] present an elaborate axiomatization based on the notion of a finite set of objects, all of whose utilities are to be measured, but since they interpolate between adjacent utility points as soon as they apply their results to measurement of subjective probability, it is difficult to discover why they confined themselves to a finite set in the first place. Their stated reason is that they wish to construct a model and derive from it predictions that can all be stated and verified. Suppes & Winet (126), who present a similar axiomatization of utility, make essentially the same argument. It is obvious that such a finitistic model is nearly useless for practical purposes; the whole purpose of a model is to carry information which permits extrapolation beyond the experimental information already available.

Nothing in the fundamental Davidson, Suppes & Siegel idea depends on the finiteness of their model or on their refusal to define indifference as 50 per cent choice; the same technique of subjectively 50-50 events could be applied (much more simply) to determination of utility points spaced along continuous utility functions. No one has, in fact, yet done that, but Coombs & Komorita (35), using this method (but without experimental verification that their objectively 50-50 event was also subjectively 50-50), have tested the hypothesis that if  $a$  is greater than  $b$  and  $c$  is greater than

$d$ , where  $a$ ,  $b$ ,  $c$ , and  $d$  are all utilities on an underlying continuous scale, then  $a + c$  should be greater than  $b + d$ . In 29 cases out of 30, it was. Similarly, Hurst & Siegel (74) tested predictions about choices among bets based on Siegel's (113) higher ordered metric technique. Siegel's metric technique, in effect, orders differences among utility intervals, and then differences among those differences, and so on up as far as the number of data points and the consistency of the data permit. Hurst & Siegel interpret their result as supporting the *SEU* model, and, further, as showing that choices opposite to those predicted by the model have longer latencies than those consistent with the model. Unfortunately, the Hurst & Siegel results are based on a data-analysis method not described in their paper or published elsewhere, and so cannot be evaluated.

Davidson, Suppes & Siegel (42), in a second experiment reported in their book, were concerned with removing the equal-spacing restriction on utilities, and also with fixing up some other difficulties in their model and first experiment. They, therefore, attempted to measure utility by a linear programming method. They used phonograph records as their valuable objects; in general, their results were encouraging to supporters of *SEU* models. But Suppes & Walsh (125) have correctly pointed out that the linear programming model violates what Savage (106) has named the "sure-thing principle." The sure-thing principle (which has also been called by other names) asserts that if course of action  $A$  is at least as good as course of action  $B$  in all possible future states of the world, and is definitely better in one or more, then  $B$  should never be preferred to  $A$ ; it is about the only universally accepted and universally empirically confirmed principle in decision theory. Suppes & Walsh substituted a nonlinear programming model, in much the same spirit as the linear programming model but with a multiplicative instead of an additive constant. Using this model, they performed an experiment which confirmed *SEU* maximization fairly well; they did not find (as Davidson, Suppes & Siegel did) confirmation of the assumptions, which are implied by the model, about probabilities of choices in cases of inconsistency. (Incidentally, they used money, not phonograph records.)

Simon (116; see also 117) has argued for a utility function with only two levels, which might as well be called good and bad. He points out that such a function can easily approximate more complicated continuous functions and greatly reduces informational and computational requirements for correct action. Such a function has never been found experimentally, but it sounds like a handy tool for approximate solution of normative problems, e.g., in systems design.

Utility models usually start with a function relating utility to amount of some commodity, or to commodity bundles. The combination of different objects, which may interact, into bundles has remained outside the theory. Adams & Fagot (1) have discussed the simplest case in which the utility of a bundle is a simple additive function of the utilities of the component commodities. A treatment of the more general problem may be hard to come by.

## RESEARCH ON SUBJECTIVE PROBABILITY

In 1954 it was already clear that expected utility maximization models were unsatisfactory and that the crucial necessary change was to replace objective with subjective probability in such models. But it was by no means clear what a subjective probability is. In 1960 it is clear what a subjective probability measure is, but it seems unlikely, in the face of the data, that subjective probabilities conceived as measures are any more adequate than objective probabilities. Less restrictive definitions of subjective probability, which do not require them to be measures in the sense of measure theory but which still preserve a form of the *SEU* model, are in much the same state of ambiguity and ill-definedness as in 1954.

*Formal properties of subjective probabilities.*—The crucial step toward clarity about subjective probability was the publication in 1954 (but too late for my earlier review) of *The Foundations of Statistics*, by Savage (106). Savage, a mathematical statistician, based his discussion on two main assumptions. One is that all acts can be rank ordered. The second is the sure-thing principle defined above. From these two assumptions, plus a number of others which have only technical importance, he developed a measure of subjective probability (he prefers to call it personal probability) for all events. Subjective probabilities have the same mathematical properties as objective probabilities, but there the resemblance ends. A subjective probability is a number that represents the extent to which an individual thinks a given event is likely. Individuals can freely choose any subjective probabilities they like, prior to the first occurrence of an event; thereafter the change in subjective probability as a result of experience is governed by Bayes's Theorem. This means that if two people observe a series of coin flips, they may start out with subjective probabilities of heads which differ widely from each other, but after a number of flips they will end up with subjective probabilities very close to each other and to the ratio of heads to total flips. This notion of subjective probability is not enough by itself to provide a complete theory of decision making in risky and uncertain situations, and the rest of Savage's theory, though elegant, is more or less a conventional *SEU* model.

A number of experiments which I reviewed in 1954, including, in particular, my probability-preference experiments (50, 51, 52), seemed to indicate that on empirical grounds the Savage kind of subjective probability measure is unacceptable. [Fréchet (68) doubted its acceptability on logical and intuitive grounds and appealed to psychologists to find out, and de Finetti (47) joined in the appeal, though insisting that a distinction be made between refusal to accept Savage's axioms and errors of judgment or calculation which might be made in applying them.] A Savage subjective probability measure requires that the sum of the probabilities of a mutually exclusive, exhaustive set of events be one. People do not behave that way; they may, for example, assign subjective probabilities greater than .5

to both the occurrence and the nonoccurrence of an event. Probabilities that do not add up to one are not measures in the sense of measure theory. I have argued (58) that the logical difficulties resulting from requiring both subjective and objective probabilities of the same events to add up to one simultaneously, combined with the experimental evidence against additivity, should lead to the abandonment of additivity in *SEU* models. A nonadditive *SEU* model, in order to be internally consistent, must measure utility on a ratio scale; the true zero point for utility is where you now are. Even a *SEU* model with nonadditive subjective probabilities implies some strong decomposability properties which can be tested experimentally.

*Measurement of subjective probabilities.*—Just as in the case of utility, there are two approaches to measuring subjective probability: direct psychophysical judgment methods, and inference from a *SEU* model. Direct psychophysical methods usually require subjects to estimate the proportion of one type of element in a display that has stimulus elements of two types. By far the most substantial study of this kind was done by Shuford (112) who used 400-element square matrices of horizontal and vertical lines as his stimuli, presented them for a brief time, and required subjects to estimate the percentage of one type of element in the matrix. The finding was that subjects performed the task extraordinarily well, producing estimates within a few percentage points of the true values. Similarly, Stevens & Galanter (122), asking subjects to make category (rating) judgments of proportion of (say) blue dots among 36 blue and green dots randomly scattered over a surface, obtained a function relating judged category to proportion that is nicely linear except for the usual distortions at the two end categories; this is consistent with a previous finding by Philip (98). I found much the same thing in an experiment like Shuford's (112). Thus, the conclusion from direct estimation experiments is that subjective probability is linearly related to observed proportion—which should please believers in rationality.

Toda (130 to 133) invented a complicated method of measuring subjective probability that uses a gambling game but is more like direct estimation than like inference from a *SEU* model. The game requires pairs of subjects to make bids that reflect their subjective probabilities. Shuford (112) applied Toda's game to his matrices. Many of his subjects gave results that look fairly linear with proportion; others did not. Shuford also attempted to test one consequence of additivity for probabilities: the probability of obtaining two favorable outcomes in two repetitions of the same event is the square of the probability of one favorable outcome. Shuford interprets his results as indicating that his subjects reflected this square law (which none of them could state) in their behavior, but his detailed plots of data are not especially convincing.

The two utility measurement experiments reported in the previous section also measured subjective probability. Davidson, Suppes & Siegel (42) measured only the subjective probability of an event whose objective prob-

ability was .25; for most subjects the subjective probability was lower than that. Davidson, Suppes & Siegel assume that the subjective probability of any event and its complement add up to one; it is unfortunate that they did not also measure the subjective probability of the complementary event and, thus, test this assumption experimentally.

I also measured subjective probability in my utility measurement experiment (56), and in doing so raised a question that is likely to haunt subjective probability theory for some time to come. Subjective probability functions obtained from bets on which subjects could only win or break even indicated that subjective probability exceeded objective probability at all points between 0 and 1. But functions obtained from bets on which subjects could only lose or break even indicated that subjective probability equalled objective probability. In other words, there was a vigorous interaction between the sign of the payoff and the shape of the subjective probability function. Of course, no such interaction is permitted by the *SEU* model. Is it possible, as Irwin (75) has suggested, that subjective probability and utility (not merely sign of payoff) interact? If so, little is left of any *SEU* model. At any rate, the interaction with sign makes it difficult to evaluate the many experiments that conclude that low probabilities are overestimated and high ones underestimated; such experiments almost always include both signs in the same bet. [For conflicting evidence see (38, 75).]

The subjective probabilities and utilities obtained in my experiment were combined to predict choices between members of pairs of equal expected value bets. The predictions were 90 per cent correct for positive expected value, 73 per cent correct for negative expected value (chance is 50 per cent).

Cohen, Hansel, and their associates (18 to 32) have conducted a very active program of research on the nature of subjective probability, the addition and multiplication theorems, the difference between skill and chance situations, and a number of related issues. Cohen & Hansel (31) recently summed up their findings on the relation between subjective and objective probability in a paragraph which nicely gives the flavor of the entire program.

... the relationship is complex and cannot be reduced to a simple formula. In certain circumstances the two types of probability tend to coincide; in other circumstances they diverge and this divergence seems to be of a systematic nature. Secondly, subjective probabilities are, in general, very much influenced by age and experience. Thirdly, the subjective probability relating to any particular preference expressed is affected by the number and value of alternatives offered.

Brim & Koenig (14), like Cohen, have concluded on the basis of a small-scale experiment that the addition theorem for subjective probabilities is not correct. Finally, McGlothlin (94) has extended Griffith's (72) earlier work on horse racing, with essentially the same results but more detail (including a rule for betting which should enable you to beat the track).

## STOCHASTIC THEORIES OF CHOICE

In 1954 the theories of choice were mostly deterministic. [But see (69).] That is, they asserted that whenever  $A$  was higher in *SEU* than  $B$ ,  $A$  would be preferred to  $B$ . The major recent theoretical development is a shift from deterministic to stochastic models, which do not generally assert that  $A$  will be preferred to  $B$  but only indicate a probability of choice between  $A$  and  $B$ . Two kinds of empirical findings, both of which were quite clear in 1954, underlie and motivate the development of stochastic models. One is the finding that a subject, required to make a choice from the same set of courses of action under the same conditions as a previous choice, may not repeat the previous choice; this is called inconsistency. The other is that sets of choices are often intransitive—that is, a subject may prefer  $A$  to  $B$ ,  $B$  to  $C$ , and  $C$  to  $A$ . Nonstochastic models formally exclude both of these empirical facts, and so usually are accompanied by some vague theory about errors. Stochastic models permit both facts but put stringent restrictions on them; these restrictions provide the most important empirical tests of the various stochastic models.

*Luce's model.*—By far the grandest and most complete stochastic theory of choice yet proposed is contained in Luce's book, *Individual Choice Behavior* (87). Luce starts by assuming one strong axiom which is now widely known as Axiom 1; the rest of the book shows how much work that axiom can do. Axiom 1 asserts (for probabilities of choice not 0 or 1) that if set  $T$  has a subset  $S$  which has a subset  $R$ , then the probability of choosing  $R$  from  $T$  is equal to the product of the probability of choosing  $R$  from  $S$  and the probability of choosing  $S$  from  $T$ . From this axiom Luce induces an underlying scale of preference which he calls a  $v$ -scale. He then proves several other theorems from Axiom 1. By far the most interesting, because of its ready empirical testability, is the one which asserts that the frequency of intransitive triads of choices of the form  $A > B > C > A$  should be equal to the frequency of intransitive triads of choices of the form  $A > C > B > A$ , which cycle in the opposite direction.

Luce next directs his attention to a series of theorems about the psychophysical applications of the  $v$ -scale. He accepts Fechner's assumption that the probability of discriminating between two stimuli is a function of the distance between the stimuli on an underlying continuum (the logarithm of the  $v$ -scale); indeed, this statement is a consequence of Axiom 1. It does not necessarily follow that either the  $v$ -scale or its logarithm is the kind of subjective scale of sensation that psychophysicists seek. Luce seems attracted by the distinction between prothetic and metathetic continua proposed by Stevens & Galanter (122) and by Stevens' power law for prothetic continua; the  $v$ -scale is one such power function, but there is a discrepancy between the exponents found in Stevens' data and the exponents predicted by Luce's model.

Finally, Luce turns his attention to utility theory. Here he introduces Axiom 2, which appeared (as did the substance of his utility work) in



earlier publications (84, 85, 86). Axiom 2 is about two gambles, each of which has the same two possible outcomes,  $a$  and  $b$ , but one of which gives you  $a$  if event  $\rho$  occurs and  $b$  otherwise, while the other gives you  $a$  if event  $\sigma$  occurs and  $b$  otherwise. Axiom 2 is motivated by the reasonable notion that the first of these gambles will be preferred to the second in either of two cases: if  $a$  is better than  $b$  and  $\rho$  is more likely than  $\sigma$  or if  $b$  is better than  $a$  and  $\sigma$  is more likely than  $\rho$ . Specifically, Axiom 2 assumes that the probability of choosing the first of these gambles in preference to the second is equal to the following:

$$P(a, b) Q(\rho, \sigma) + P(b, a) Q(\sigma, \rho)$$

where  $P(a, b)$  is the probability that  $a$  will be preferred to  $b$ , and  $Q(\rho, \sigma)$  is the (perhaps subjective) probability that  $\rho$  will be considered more likely than  $\sigma$ . Notice that this expression, which looks like something that you might derive from a *SEU* model, actually is not; it refers to probabilities of preference of  $a$  to  $b$  and to probabilities that  $\rho$  is considered more likely than  $\sigma$ ; neither of these kinds of probabilities can be at home anywhere except in a stochastic model. Nevertheless, there is a strong flavor of the *SEU* model in Luce's Axiom 2, except that it is, according to Luce, much weaker. (Considering the consequences of Axiom 2, this statement deserves a raised eyebrow.)

From Axioms 1 and 2, taken in conjunction, flow a remarkable series of theorems about the limits on the classes of imperfect discriminations permitted by the axioms; the most remarkable (which requires additional but trivial axioms) asserts that if any cases of imperfect discrimination ever occur among gambles, they must occur in clusters, each of which is at a specified probability of choice. In other words, the function relating probability of choice to some systematic variation of amount or probability of payoff in gambles must always be a step function. This statement seems very unlikely, but apparently Luce finds it plausible since he has done an experiment (still unpublished) that he interprets as supporting the prediction. Luce's book goes on to apply the  $v$ -scale and Axiom 2 to learning, deriving several stochastic learning models with interesting properties; this review will not follow him there.

Abundant evidence, some of which Luce discusses in his book, shows that Axiom 1 is not always correct. Many other stochastic decision theorists consider it too strong, preferring to base their theories on a stochastic equivalent of the notion of cardinal utility. But however vulnerable Axiom 1 and the models erected on it may be, Luce has provided us with one model which certainly will last: his book. Future books of psychological theory (at least in the more mathematizable areas) will have to take serious responsibility for all the logical consequences of assumptions made and for the relationships between their own theories and others, as this book does. (Perhaps these future books will contain somewhat more discussion of data.)

*Stochastic transitivity.*—Davidson & Marschak (40) generated a somewhat weaker stochastic model, based on a slight weakening of the stochastic equivalent of the notion of cardinal utility plus the notion of the subjectively 50-50 bet used by Davidson, Suppes & Siegel (42). [For closely related work, see (44, 45, 46).] Stochastic choice models may, depending on the strength of their assumptions, predict either of two kinds of stochastic transitivity. Weak stochastic transitivity simply asserts that if the probabilities of preferring *A* to *B* and *B* to *C* are both equal to or greater than .5, the probability of preferring *A* to *C* is also equal to or greater than .5. Strong stochastic transitivity asserts that if the probabilities of preferring *A* to *B* and *B* to *C* are both equal to or greater than .5, the probability of preferring *A* to *C* is equal to or greater than the larger of the other two probabilities. Davidson & Marschak (40) predict strong stochastic transitivity from their model both for utilities and for utility intervals; they performed an experiment on choices among bets to examine these predictions. They found percentages of intransitive triples ranging from 7 per cent to 14 per cent, and were easily able to interpret the finding as confirming both predictions. As a final touch, they compared *SEU* maximization, objective expected value maximization, and variance preferences (see below for definition) as predictors of their data, and found *SEU* much the best, with 81.6 per cent correct predictions (50 per cent would be chance).

The spirit of this acceptance of strong stochastic transitivity is much like the spirit of Papandreou's similar acceptance [see (96); his experiment was discussed in 1954 on the basis of a prepublication draft]. That is, both papers report experiments in which transitivity seems quite likely to be true, find an acceptably low percentage of intransitivities, and, in effect, accept the hypotheses they set out to accept. No experiment yet reported has created conditions deliberately designed to be unfavorable to transitivity, strong or weak, and ended up accepting even weak stochastic transitivity. In short, as a basis for psychological theorizing, algebraic transitivity is dead, and stochastic transitivity, strong or weak, has yet to be exposed to the adverse climate of hostile experiments. It seems likely that conditions can be designed in which subjects choose intransitively most of the time (unpublished research so indicates); it is even possible that the direction of intransitive cycles can be controlled by experimental manipulation. If so, the question for experimenters to answer is not whether any form of transitivity holds, but rather under what circumstances do various assumptions about transitivity hold, and under what circumstances do they not. Flament (63) has made a beginning toward a theoretical attack on this question, but we really have not advanced much beyond May's (92) speculation that intransitivities occur when inconsistent evaluative dimensions must be simultaneously used in evaluation.

In another experiment directly concerned with transitivity, Davis (43) repeated previous experiments that had found significant numbers of intransitive triads of choices. He, too, found many intransitive triads, but

argues that they were attributable to random choices between pairs of indifferent alternatives. His basis for this argument seems to be that subjects, given the same set of choices a second time, do not repeat the same intransitivities very often (but produce different ones instead). However, his own data raise serious doubts that his subjects were indifferent among the objects involved in intransitive triads—though neither experimental nor statistical procedures encourage the reader to conclude much of anything from this study.

An experiment by Coombs (33, 34) attacks the question of intransitivity from a different angle. He showed sets of four patches of grey to his subjects and asked them to rank order each set in preference. He found that his data did satisfy weak stochastic transitivity, but did not satisfy strong stochastic transitivity. However, he argued that if a subject has an "ideal" grey, and if a set of three greys are not all on the same side of the ideal, then strong stochastic transitivity need not apply; the underlying interval scale is "folded" at the ideal, and strong stochastic transitivity need not apply across the fold (essentially because of possible moment-to-moment variation in the location of the folding point). It is hard to see, however, how to apply this defense to the many findings that violate even weak stochastic transitivity.

Other stochastic models exist; for example, Audley (5) has one concerned with decision times.

How well you like the stochastic models depends mostly on whether or not you like to think of choice as a probabilistic phenomenon. It could be argued that inconsistencies and intransitivities should be eliminated from data by careful experimentation, or else explained deterministically by more detailed theories about choices, and not accepted and embedded in probabilistic models. But no one has yet been able to use his experimental or theoretical ingenuity to eliminate them, nor are any signs of impending success currently visible. Consequently, it seems likely that stochastic models will become more and more popular—at least among mathematically sophisticated theorists. Their popularity among experimenters remains doubtful.

#### APPLICATION OF STATIC DECISION MODELS

Though the *SEU* model has not been applied as yet, numerous applications of the notion of maximizing expected value exist. One, which amounts to a theory in itself, is the application of signal-detectability theory to human behavior in psychophysical experiments by, especially, Tanner and his associates (e.g., 127, 128). Tanner's theorizing centers on the ideal observer, who maximizes expected value in discriminating signals from noise. Many articles reporting work in this context have been published; the more important have been summarized, integrated, and evaluated by Licklider (82).

An important application of expected value maximization is to trouble

shooting of electronic equipment. Gluss (70) has worked out the mathematics for cases in which only trial replacements are possible, and Stolurow *et al.* (123) have shown that people do not follow the model. The famous half-split principle, which asserts that checks should be made so that each eliminates half of the still-admissible alternative malfunctions, is another application of expected value maximization. Goldbeck *et al.* (71) have shown that people can learn to use this technique only with very simple networks. Dale (39) has examined situations appropriate to both of these kinds of trouble shooting, with results unfavorable to optimization models. Detambel (48) and Detambel & Stolurow (49) have shown that although optimal behavior may not occur in simulated trouble shooting, changes are usually in the direction of optimality. Finally, Williams & Hopkins (137) have attempted to analyze an interceptor pilot mission in decision-theory terms.

Other applications of static decision theories will probably occur, especially as the probabilistic nature of military information-processing and decision-making systems becomes increasingly recognized and the probabilities and values which control the decisions are displayed and used explicitly, instead of being used implicitly as is the case now. But static decision theories have only a limited future. Human beings learn, and probabilities and values change; these facts mean that the really applicable kinds of decision theories will be dynamic, not static.

There is economic and administrative literature on decision making. Wasserman & Silander's (136) annotated bibliography leads into it; Simon's chapter on administrative decisions in his book *Administrative Behavior* (118) is a systematic treatment of some of the real-life problems (e.g., what set of possible courses of action does the decision maker really consider) which theories tend to gloss over. In a similar spirit is Lichtenberg & Deutsch's (81) review of research on military-staff decision making. Finally, there is the concern of economists with real-life economic decision making; an example is a symposium on expectations, uncertainty, and business behavior (11) held in 1955. A British economist named Shackle has developed a decision model which purports to be especially concerned with such problems; a symposium in which that model figured extensively has been published (15). However, that model turns out to be the nonadditive *SEU* model in a wildly different guise; for a demonstration, see (57).

#### VARIANCE PREFERENCES

A recurrent theme within static decision theory has been dissatisfaction with *SEU* models, stochastic or otherwise, because they fail to take into account important properties of static choices. The most common form of this complaint, already clear in 1954, is that the variance of a bet is as important as its *SEU* in determining its attractiveness. Three major experimental attempts to examine the existence of variance preferences have been

made. In 1954 I conducted an experiment (54) using the general technique of previous probability-preference experiments (50, 51, 52). Subjects were required to choose between pairs of zero expected value two-outcome bets; one member of each pair had high variance and the other had low variance. Furthermore, conditions were designed to favor high-variance choices at some times and low-variance choices at other times. Little effect of variance on choices appeared. The conclusion from this experiment was that variance preferences are, at most, a second-order determiner of choices.

Royden, Suppes & Walsh (105) performed an experiment comparing sure things with 50-50 bets, in an attempt to measure the utility of gambling as an activity. Insofar as the notion of "utility of gambling" can be given an operational meaning, it must correspond with variance preferences since, clearly, any choice whose outcome is uncertain will have a higher variance than the (zero) variance of not gambling at all. Unfortunately, their results indicate a marked utility for gambling only on the assumption that utility of money is linear with dollar value; a nonlinear utility function predicts the results of the experiment somewhat better than the notion of variance preferences. This is the first explicit example of the inevitable confounding between utility and variance which must exist in all experiments using two-outcome bets; the same criticism applies to my variance preference experiment, and still another example follows.

Coombs & Pruitt (36) have performed a large experiment concerned with variance and skewness preferences. They prepared a square array of two-alternative bets, all of zero expected value, with variance on one dimension and skewness on the other. Variance in such an array is (for any single skewness level) completely confounded with the magnitude of the difference between outcomes; skewness is completely confounded with probability of winning; therefore, these names for the two dimensions of the table are not unique. They examined preferences within certain rows and columns of this table by means of the method of paired comparisons. The choices were hypothetical, but subjects played one bet at the end of the experiment. Data failing to satisfy weak stochastic transitivity were rejected; data from the remaining subjects were analysed according to Coombs's unfolding technique. The major findings were that most subjects chose consistently and transitively and that most subjects exhibited single-peaked variance preferences and (except for an undue preference for 50-50 bets) single-peaked skewness or probability preferences. The consistency found by Coombs & Pruitt was extraordinarily high; perhaps it is because their subjects made their choices among bets at rates ranging from 3.4 to 22.4 choices per minute, and so could not have taken much time for reflection. The two rules, "Always choose the bet with the highest payoff for winning" and "Always choose the bet with the lowest cost for losing," together account for about 68 per cent of all the rank orderings Coombs & Pruitt obtained. These rules, of course, could be applied simply and mechanically to minimize reflection.

Variance preferences are necessarily confounded with utility, and skewness preferences with probability, for two-alternative bets. Therefore, all research on variance preferences so far is ambiguous. The remedy is to use more than two alternatives; experiments which do so are in progress.

#### PERSONALITY VARIABLES IN DECISION MAKING

In 1954 there was little or nothing to say about how personality variables might influence decision making, except that the theory of level of aspiration which had been developed by Lewin and his collaborators (80) had a substantial resemblance to the *SEU* model. By 1960 a great deal of work on personality variables in decision making has been completed, most of it expressing Lewinian or Lewin-influenced points of view.

The most important modern version of this point of view is that of Atkinson, who has discussed in detail his model for risky decision making. Atkinson (3) is primarily concerned with the motive to achieve success ( $M_s$ ) and the motive to avoid failure ( $M_f$ ). He is also concerned with the subjective probability of achieving success ( $P_s$ ), and of failing ( $1 - P_s$ ). Finally, he is concerned with an incentive value of achieving success and an incentive value of avoiding failure; his crucial assumption is that the former is inversely related to the subjective probability of succeeding (and so is given by  $1 - P_s$ ) and the latter is the negative of the subjective probability of succeeding ( $-P_s$ ). He does not do the algebra implied by these definitions, but resultant motivation, as he defines it, is given (after appropriate algebra) by  $P_s (1 - P_s) (M_s - M_f)$ . For comparison, the variance of a two-alternative bet in which you win  $A$  with probability  $p$  and win  $B$  with probability  $1 - p$  is  $p (1 - p) (A - B)^2$ . The major difference between the two formulas is that Atkinson's quantity can be negative since the difference term is not squared. Nevertheless, it looks as though there should be a close resemblance between Atkinson's theory and a theory about variance preferences, and indeed there is.

Atkinson proposes that there are two kinds of people, those in whom the motivation to achieve success is greater than that to avoid failure and those in whom the reverse is true. It is obvious that the function with which Atkinson is concerned has a maximum at  $P_s = .5$  if  $M_s$  is greater than  $M_f$ , and a minimum there if  $M_f$  is greater than  $M_s$ . Consequently, Atkinson predicts that subjects of the first kind will prefer bets of intermediate probability of success (and consequently relatively high variance), while subjects of the second kind will prefer bets with probability of success near either 1 or 0 (and consequently relatively low variance). After reviewing experiments that he interprets as showing this, he suggests, on the basis of the findings, another assumption: "The relative strength of a motive influences the subjective probability of the consequence consistent with that motive—i.e., biases it upwards" (3, p. 367). He finishes with a discussion of the effect of success or failure on subjective probability.

Atkinson *et al.* (4) tested Atkinson's model with a shuffleboard experi-



ment in which fictitious probabilities of success were displayed to subjects. Subjects with high  $M_s$  stood closer to the target than those with low  $M_s$ , and thereby more nearly achieved the intermediate probability levels they are supposed to like. Also, in a replication of my probability preference experiments everyone preferred the sure thing, but after that was excluded from the analysis the subjects with high  $M_s$  preferred 2/6, 3/6, and 4/6 probabilities, while those with low  $M_s$  preferred 1/6 and 5/6. [See also (93).]

More closely related both to traditional Lewinian theory and to *SEU* maximization is Siegel's (114) discussion of level of aspiration. On a priori grounds, Siegel defines level of aspiration as the least upper bound of that chord connecting two adjacent points on a (discontinuous) utility scale that has the maximum slope. Siegel does not point out that this definition is meaningful only for independent variables measured on at least an interval scale; otherwise the concept of slope is not defined. Becker & Siegel (8) performed an experiment in which they actually gambled with midterm grades in a psychology course as prizes. (Presumably grades are ordinal-scale independent variables; Becker & Siegel apply Siegel's definition simply by assuming them equally spaced.) They interpreted the results as confirming Siegel's conception of level of aspiration.

Rotter's social-learning theory (103, 104) has inspired a number of doctoral theses (e.g., 83, 97, 139) which fit vaguely within the framework of the *SEU* model. Brehm (13) did a choice experiment which he interpreted in terms of Festinger's cognitive dissonance theory (62). Block & Petersen (10) characterized decision makers on the basis of various psychological tests; mature subjects made mature decisions. Feather (60, 61), following Lewin and Atkinson, supposes that the harder an objective is to attain, the higher will be its utility, and reports an experiment that he interprets as supporting the hypothesis. Proponents of this hypothesis have not distinguished between two things it might mean. One possibility is that the world is so constructed that more valuable objectives are, in fact, usually harder to attain; this is obviously true but does not require postulation of any special psychological interaction between utility and subjective probability. The other possibility is that the same goal becomes more attractive if it becomes harder to attain; this, if true, requires modification of customary *SEU* models.

Scodel, Ratoosh & Minas (110) required their subjects to choose among bets. They found that expected value did not control preferences and reported phenomena analogous to variance preferences. Subjects who chose more conservative bets were higher on need achievement, theoretical and aesthetic values, and fear of failure than those who chose more risky bets.

No coherent picture emerges from this area of study as yet. It is surprising that so few studies explicitly examine utility and subjective probability functions, relating their shapes in different people to personality variables.

## DYNAMIC DECISION MAKING

In real life, decisions occur in sequences and information available for later decisions is likely to be contingent on the nature and consequences of earlier ones. The study of decision processes in such changing situations might be called the study of dynamic decision making. Two cases can be distinguished. In one, the most frequently studied, the environment is (stochastically) unchanging, but the decision maker's information changes as a result of successive decisions, other events, or both. In the other, little studied because it is so complex, the environment changes its characteristics while the decision maker is obtaining information about it.

The distinction between dynamic decision processes and learning is one of emphasis, not content. In particular, probability learning experiments examine a very simple case of dynamic decision making; such experiments are omitted here to avoid overlap with other chapters.

*Intuitive statistics.*—If the environment is stationary but man's information about it is changing, then a decision task is likely to look very much like a problem in statistics. In fact, most statistical tests can be treated as mathematical models for human behavior, and people can be examined to see if they in fact conform to these models.

Irwin and his collaborators have exploited this possibility in a series of experiments on what they call the "expended judgment" situation. Subjects are presented with a sequence of cards, on each of which a number is written. Statistical properties of the sequence of numbers are varied, and subjects are required to make judgments about the population which the numbers represent. In the first of these experiments (78) subjects judged, after seeing each of 20 cards, whether the mean of a 500-card deck was greater or less than zero and also indicated their confidence in their judgments. The mean confidence ratings (algebraically added so that they also represent judgments of greater or less than zero) were directly related to the mean of the sample, and the absolute magnitude of the confidence ratings increased with sample size and decreased with sample variability. Another part of the same experiment confirmed these results in a slightly different situation in which cards from two decks were simultaneously exposed, and the subject judged which deck had the larger mean. In another experiment (76) subjects were required to look at cards until they had seen enough and then stop; they stopped sooner when the means were more different and later when variability was larger. In yet another experiment (77) subjects were paid for correct decisions and charged for each card looked at; greater cost and lower prize reduced the number of cards looked at, whereas lower cost and greater prize increased that number. In addition, the findings of the previous experiment concerning difference between means and amount of variability were confirmed. Unfortunately, it is impossible to calculate whether subjects were or were not adopting an expected-value maximizing strategy, or something like it. For one thing, a

peculiar payoff scheme was used which meant that subjects could not tell the relationship between the amount of imaginary money they had won or lost and the amount of real money they would eventually be paid. Furthermore, subjects had no information about the distribution from which they were sampling, and, consequently, no optimal strategy can be calculated (except for one unreasonable one based on the peculiarity of the real payoff scheme).

Becker (7) was also concerned with optional stopping, but he used an experimental design in which the population sampled was binary. Since the population distribution is known except for one parameter, it is easy to calculate optimal strategies in the sense of statistical decision theory. Becker used Wald's model for a two-action decision problem with optional stopping, and compared the behavior of his subjects with the model. He concluded that although people did not perform exactly as the model would require, they came pretty close. He found consistent individual differences that are interpretable as variations in parameters of the model. Unfortunately, Becker used a nonlinear and peculiar payoff scheme similar to the one used by Irwin & Smith (77); it remains unclear what effect variations in real payoff might have on performance.

The upshot of these studies of man (or rather, college student) as statistician is that he makes a fairly good one. In all cases the differences are in the proper directions, though they are not always the proper amounts. (The findings of direct probability estimation experiments discussed earlier are similar.) Of course the only task studied in these experiments is the estimation of the mean. It is an interesting experimental question whether man can perform other statistical tasks well on an intuitive basis. It seems unlikely, for example, that men can estimate variances as accurately as they can means; in fact, some of the minor findings of the Irwin, Smith & Mayfield (78) experiment suggest this.

#### EXPERIMENTAL GAMES

The theory of games has progressed vigorously as an area of mathematical investigation; it would take a book to do justice to its development. Fortunately, just such a book is available, and an extraordinarily thorough and clear one at that: Luce & Raiffa's *Games and Decisions* (88). This review will make no attempt to cover the material on games which Luce & Raiffa have so ably covered; instead, it will be confined solely to experiments on game playing in the spirit of the theory of games. However, attention should be called to discussions of game theory quite different in spirit from Luce & Raiffa's (and quite critical) by Schelling (107) and Rapoport (101), to Braithwaite's curious and ingenious attempt to apply game theory not to behavior but to ethics (12), and to a brilliant exposition by Milnor of the meaning of and relations among various decision criteria which are often spoken of in game-theoretical discussions (95; see also 16 and 99 on the same topic).

*Bargaining games.*—Most experimental research on games has been concerned with simple bargaining games. A number of experiments by Flood were reviewed in 1954 on the basis of unpublished reports; some of them have now been published (64 to 67) along with some other games of the same sort. The main finding from these anecdotal experiments was that people tended to adopt what Flood calls a split-the-difference principle, which distributed gains equitably among participants. Hoffman, Festinger & Lawrence (73) studied a three-person game in which two subjects and a stooge competed for points. The stooge was given an initial advantage. The main finding was that all subjects sought equity by bargaining in such a way that the stooge was thereafter at a disadvantage; to do so, subjects refused offers which they might profitably have accepted. This tendency was more pronounced under conditions of high task importance ("It is an IQ test") than under conditions of low task importance, and more pronounced when the stooge was perceived as an equal than when the other subjects thought that the stooge was unusually intelligent (because of some ingenious trickery). Kalisch *et al.* (79) similarly found that in a variety of games their subjects were reluctant to exploit advantageous bargaining positions and tended, instead, to distribute proceeds as equitably as the rules of the game permitted. Finally, Stone (124) performed an experiment in which subjects unknowingly played against themselves in a variety of well-displayed non-zero-sum games which did not involve negotiation, haggling, or any element of bargaining other than a simple offer. Stone's selection of interesting games is wide, and his display technique has intuitive appeal—in short, it looks as though Stone's methods for studying one-offer bargaining approach optimality. Unfortunately, he threw out 61 per cent of his subjects for violating the sure-thing principle one or more times. From the remaining data he concludes only that people who are conservative in some situations are also conservative in others, and that the mathematical models for his particular kind of situation do not fit very well. This startlingly large percentage of rejections probably results from the fact that Stone's subjects had to make rather difficult imaginary judgments at the rate of about one a minute, with no informative feedback. Some of Stone's data also suggest that his subjects pursued equity, though he does not report his results in enough detail to permit any thorough examination of them.

Game theory is not the only kind of theory that can be tested by means of two-person games. Siegel & Fouraker (115) have investigated the traditional economic problem of bilateral monopoly (one commodity, one buyer, one seller) for which a number of models, including the game-theoretical one, are available. Many of these models have in common one important prediction: although the amount of the commodity exchanged will be relatively determinate at the quantity which maximizes joint payoff, the price will not be. Siegel & Fouraker designed an elaborate experiment, in many ways a model of how such experiments should be done, in which pairs of subjects bargained (under very restricted communication rules, and without

personal contact or awareness of each other's identities) over prices and quantities of a hypothetical commodity, but with real payoffs contingent on success in bargaining. Their main finding was, as the theories had predicted, that the quantity was determinate at the quantity which maximized joint payoff (and the same for different pairs of subjects) but that the price was not. In spite of the restricted communication enforced by the experimental design, the pursuit of equity discussed above emerged in this experiment also.

On the average, negotiated prices do not vary significantly from that price which is associated with a fifty-fifty division of the maximum joint payoff. Dispersion of negotiated prices around the even division price is reduced as the amount of information is increased, approaching the limit under complete information, a condition under which most contracts represented fifty-fifty splits of the maximum joint payoff (115, pp. 75-76).

Furthermore, if one member of a bargaining pair knew the costs, prices, and profits of both, while the other knew only his own costs, prices, and profits, then the member with more information was at a disadvantage, because he more quickly arrived at the equitable offer and, consequently, was at a disadvantage in subsequent bargaining.

*The prisoner's dilemma.*—One finding contradicts the hypothesis of desire for equity. Scodel *et al.* (109) examined a fascinating non-zero-sum non-co-operative game called the prisoner's dilemma [see Luce & Raiffa (88), pp. 94-102]. In this frustrating game, if both players choose their optimal strategy, each gets less than if both had chosen a nonoptimal strategy. Scodel *et al.* found that players overwhelmingly tended to choose their optimal but less well-paid strategy, and other unpublished research confirms this finding. Furthermore, players become more, rather than less, addicted to this un-co-operative response as the game progresses. The structure of the prisoner's dilemma makes it very difficult for equity-seeking motives to operate; they almost certainly lead to substantial and inequitable penalties unless they operate in both players at exactly the same time. Scodel *et al.* find that if the players are permitted to discuss the matter, they thereafter do better at choosing the nonoptimal but financially more desirable pair of strategies, and, of course, if this were an unrestricted bargaining game, they would easily come to the two-player coalition which would permit them to extract the maximum return from the experimenter. Restraints on communication seem essential to prevent the equity-seeking motive from operating effectively, even in a situation which punishes equity-seeking as effectively as does the prisoner's dilemma.

The main finding from these studies of multiperson games seems to be that people import into bargaining situations a strong desire for equity. Equity seeking is promoted by effective and free communication and seriously hindered, or even prevented, by severely restricted communication. Equity seeking produces results in conflict with those implied by game

theory and similar theories about rapacious economic men, except in those games in which equity seeking and uninhibited rapacity have the same consequences. If this finding stands up under more experimentation, especially with much larger payoffs, theories about multiperson decision situations must either be modified to incorporate such social motives explicitly, or else some means for incorporating them in utility functions must be found.

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